

THE
MONTHLY
JOURNAL OF FOREIGN MEDICINE.

OCTOBER, 1828.

From the Transactions of the Medico-Chirurgical Society
of Edinburgh.

ADDITIONAL CASES AND OBSERVATIONS ILLUSTRATING THE ORIGIN OF TUBERCLES. By W. P. ALISON, M. D., F. R. S. E., &c. and Joint Professor of the Institutes of Medicine in the University of Edinburgh.

The inquiries on the subject of scrofulous diseases, of which I formerly laid an account before the Society, were directed chiefly to the elucidation of the two following questions: 1st, Whether the scrofulous diathesis, or disposition to scrofulous disease, unfortunately so prevalent in our climate, is to be regarded as the result of the climate alone; or whether other and more remediable circumstances in the mode of life of those who become so affected, contribute generally and powerfully to its production; and 2dly, Whether the deposition of tubercles, so often the original cause of danger and death in scrofulous diseases, is to be regarded, in any case, or in any considerable number of cases, as an effect of inflammatory action.

On the first of these questions, I have since obtained information from various quarters, tending to confirm me in the opinion, that in regard to the tendency to scrofulous diseases, and particularly to those in which tubercles are formed, there is a greater difference between the inhabitants of large towns and of the country, than between the inhabitants of the warm and cold climates, at least of Europe; and therefore, that the general prevalence of the scrofulous diathesis is to be ascribed, rather to the modes of life which an advanced and artificial state of society implies, than to the circumstances of climate. But the information which I have collected on this head, is not yet so ample and satisfactory as I hope to be able to make it, before submitting it to the Society.

In regard to the second point, I should wish it to be remembered, that my object was, not to explain the whole history, and all the possible causes of tubercles, but only to investigate the question, which seemed of the greatest practical importance, whether or not inflammatory action has, in any circumstances, the power to produce them? And on this subject I cannot help remarking with satisfaction,

that although, at the time when I wrote my former paper, the most celebrated authors and teachers in the school of Paris, who had studied this subject minutely, Bayle, Laennec, Rostan, Louis, Velpeau, &c. were disposed to allow little or no influence to inflammation in the production of tubercles; yet those of the French pathologists who have attended most minutely to the subject since that time, particularly Professors Andral and Cruveilhier, have adopted an opinion almost identical with that which I endeavoured to explain.*

The testimony of Andral is the more valuable, as his previous opinion in regard to the formation of tubercles appears to have been nearly the same as that of Laennec. The following sentence in the third volume of his *Clinique Medicale* expresses very nearly the opinion which my own observations have led me to form, regarding the connection of tubercular deposition which increased vascular action. "If the disposition to the formation of this new product is very strong, then the slightest local congestion of blood is sufficient to give occasion to it; wherever such congestion takes place, the same product appears; or what is called the tubercular diathesis is produced. If this disposition is less strong, it is requisite for the formation of a tubercle that the congestion of blood should be so considerable, and so permanent, as to amount to inflammation. But, when there exists no such predisposition, the most intense, and the longest continued inflammation, will not produce a tubercle."† For the last clause of this sentence, I should be disposed to substitute, "the kind of inflammation which is most generally excited has no tendency to produce tubercles." He remarks, in another place, that the condition which seems most conducive to the deposition of tubercular matter from inflammation, is not its intensity, but its long duration.

* See Andral *Clinique Medicale*, t. iii. p. 13, *et seq.*; also an excellent "Essai sur les Tubercles," by a gentleman who has distinguished himself by his attention to the subject both at this school and at Paris, M. Lombard; and Cruveilhier's paper in the *Bibliothèque Medicale*, September 1826.

† T. iii. p. 24.

I shall now take the liberty of directing the attention of the Society to the facts which have occurred to me, both in the way of reading and of observation, since my former paper was read, and which seem to me to concur in establishing the proposition, that in certain constitutions, inflammation, acute or chronic, but most generally chronic, does frequently and directly lead to the deposition of tubercles.

I think it an important point, in the outset of this inquiry, and one which we may consider established by the researches of some of the pathologists of Paris, that, in so far as the most minute anatomical observations can inform us, tubercles are very seldom found in the bodies of children who are still born, or die very shortly after birth. On this point I trust chiefly to the statements of Dr. Denis, lately Eleve Interne at the *Enfants Trouvés* at Paris. In the "*Recherches d'Anatomie et de Physiologie Pathologiques sur plusieurs maladies des Enfants nouveau nés*" of this author, we have a detailed account of 139 dissections of still-born, or very young children, which took place in the Hospice, under the eye of the author, in 1823; and after these extensive opportunities of observation, and after having attended minutely to the statements of former observers, he was persuaded, that where they had described tubercles in the lungs in the foetal state, or very soon after birth, they had been mistaken; and thought himself justified in asserting that, "*On n'a pas encore trouvé jusqu'ici de tubercules pulmonaires avant les premiers mois que suivent la naissance; on commence seulement à les rencontrer dans des enfans de 5 ou 6 mois.*"* He then goes on to state (what agrees with my own observations,) that even at this last age "*les tubercules ne sont que rarement sous forme miliaire chez les enfans; leur substance infiltre ordinairement une masse considérable des poumons, ou seulement les glandes bronchiques.*" And in speaking of the abdominal diseases of new born children, he states equally decidedly, "*Jamais, chez les nouveau nés, et meme rarement avant la première dentition, l'engorgement (des corps lymphatiques) ne se termine par une induration remarquable, par suppuration, ou par une dégénérescence tuberculeuse. Le Carreau (Tabes mesenterica) est une maladie qu'on doit rayer du cadre des affections des enfans en bas age.*"†

To the same purpose I find it stated by Velpeau, that both he and Breschet, (surgeon to the *Enfants Trouvés* at Paris,) had frequently sought for tubercles in the foetus, and never found them; and that though other observers, particularly Orfila and West, had seen them, it was only in a small number of cases.‡

We must, therefore, as I apprehend, necessarily suppose, that in by far the greater number of the very numerous cases in which tubercles are found in bodies of young children, the diseased actions by which they were form-

ed had originated after birth, and seldom sooner than some months after birth,—parents transmitting to their offspring only the tendency to this kind of diseased action, and very seldom the actual disease. I would next recall to the recollection of the Society the observation of Magendie, that in those cases in which he had detected tubercles of the smallest size, and apparently in the earliest stage of the bodies of young children, they were surrounded by circumscribed vascularity. This I have myself observed, not uniformly, but certainly in a great many cases. I formerly stated likewise, that in cases of young children, which have come under my own observation, and where I was accurately informed of the whole progress of the symptoms, when tubercles have been found in unusual numbers, and been apparently the cause of unusually rapid emaciation, and early death, the first and chief symptoms of the disease have been decidedly those of inflammation, from which the patients never recovered, and of the consequences of which, judging from the symptoms, it was obvious that they died.

The two following cases, which have occurred to me since my former paper was printed, seem to me striking illustrations of this general fact.

1. Allan Mackintosh, a child aged two years and two months, living in a confined and ill-aired part of the town, had nevertheless been healthy from his birth,—had not had any of the contagious diseases of children, or any sickness of a day's duration, and had never had a cough, until the night of the 12th May 1824, when, after having been taken to some distance from town, and exposed to cold, he was seized with heat, restlessness, cough, and short breathing. I saw him on the 13th, after he had been prescribed for by one of the pupils attending the Dispensary. His pulse was then very quick and full; his face flushed, skin hot, and breathing much oppressed. Four leeches were applied that evening, and again the next day; these bled freely, and the strength of his pulse and the heat of his skin were much diminished. The pneumonic symptoms continuing, however, in a less degree, leeches were again applied twice within a few days after this date; he also took laxatives, and on several days repeated doses of a solution of tartar emetic. About the end of the first week of his disease, the pectoral symptoms had abated very much, but his breathing continued short, though not difficult, and he had still cough, not severe, and generally dry. His pulse was still very quick, and rather feeble. His bowels at this time were rather obstinately costive for a few days, and appeared to be pained at times. He had also slight spasms, and a tendency to coma, so as to excite suspicion of hydrocephalus, but these symptoms gradually went off, and the alvine evacuations became nearly natural. After this he had frequent irregular fits of chilliness, succeeded by heat and thirst, but without sweating. His tongue was generally rather dry, his appetite never returned, and he be-

* Op. cit.

† Op. cit. p. 118.

‡ Thesis ad Aggreg. p. 10.

came weak and emaciated. In the beginning of the fourth week of his illness, his breathing became more short and difficult, and leeches were again applied, without any good effect, the dyspnœa continuing till his death, which took place on the 8th of June, the twenty-seventh day of his disease.

On dissection, the left lung appeared much condensed, and had on its surface many flakes of lymph, of a light yellowish colour, and disposed in irregular forms. When cut into, a part of its condensed substance had the common appearance of hepatization, but the greater part consisted of yellowish matter, partly diffused or "infiltré" through the substance of the lung, partly disposed in roundish tubercular masses, and partly also in long irregular flakes, which had exactly the same form, and were composed apparently of exactly the same substance, as those already noticed on the surface of the pleura.

Several of these tubercles, of the different forms now stated, were softened, and contained pus in their centre, and some of them were nearly emptied.

The right lung appeared unusually voluminous; there was some partial hepatization, but with hardly any deposition of tubercles in its lower lobe.

The other viscera of the thorax and abdomen were healthy.

2. Isabella Leslie, æt. nine months, though of a family in which there had been one death from consumption, was a healthy and lively child, and never had a cough until the 20th March 1826, when she first became feverish, after having been exposed to the contagion of pertussis. She died on the 5th April, after seventeen days of illness. I did not see the child until some days after the illness began, and the symptoms which appeared to me at first the most urgent, were a tendency to stupor, with retching; the skin was hot, and the pulse frequent, but rather feeble. The child was leeches and purged, and the comatose tendency abated, but the cough then became frequent, with short hurried breathing, and soon assumed the peculiar character of whooping cough. The heat of skin continued, though with remissions, and with more evidence of debility than is usual in the inflammatory stage of pertussis; on which account evacuations were not carried to such an extent as perhaps they ought to have been. A few days before death the cough again abated, the dyspnœa continuing, with feeble pulse, and occasional spasms.

On dissection, the upper lobes of both lungs were found somewhat emphysematous, and a considerable part of the middle and lower lobes of both was found condensed, so as not to crepitate under the knife. The effused matter producing this condensation was in some places of a reddish colour, but the greater part of it was of a grayish white. In general no distinct line of separation could be traced between the portions that had these different colours. The whitish matter was disposed partly in irregular diffused masses, but partly

also, both under the pleura and in the substance of the lungs, in little nodules, more or less distinctly circumscribed, and which had just the usual appearance of incipient or miliary tubercles. Some of these nodules had a yellowish colour, and at one spot there was a small cavity containing yellow pus. In a few places there was a little deposition of lymph, of irregular form, on the outside of the pleura; and this matter, on minute examination, appeared identical with that which had the form and appearance of the incipient tubercles beneath the pleura.

The only other morbid appearance was a quantity of frothy mucus in the bronchia.

I shall only observe of these cases, that they appear to me to comprise *both* the circumstances which I formerly stated as indicative of the dependence of tubercles in many cases for their existence, or at least for their chief properties, or inflammatory action. *1st*, The patients, previously quite healthy, suffered an attack of acute inflammation, from a known cause, very recently before death, and never recovered from that attack, but died manifestly of its consequences. A large deposition of tubercular matter was the chief appearance on dissection, and the disorganization produced in this way was so extensive, that it was quite impossible to suppose it to have existed previously to the inflammatory attack, when the patients appeared to be in perfect health. *2dly*, The tubercles appeared closely connected with, and graduated insensibly into, the usual and acknowledged effects of inflammation. The tubercular matter which was diffused through the substance of the lungs, passed by insensible degrees, as I have seen in many other cases, into the adjoining hepatized induration; and the lines of tubercular matter in the substance of the lungs appeared so precisely similar to the contiguous flakes of lymph on the pleura, that I think no one could reasonably ascribe to them a separate and wholly different origin; while these last, again, had so much the common appearance of inflammatory exudation on that surface, that I think before the lung was laid open, no one could in either case have hesitated about calling them the effects of inflammation.

In the following case, which is almost precisely similar to one formerly communicated to the Society, the symptoms were those of acute hydrocephalus, and the appearances found were such as indicated clearly inflammation of the brain and pia-mater. The effusion on this last, however, though composed every where of the same *substance*, had in some points the usual *diffused form*, and in others the *form of tubercles*.

Pitcairn Smith, æt. thirteen months, December 1825. Had had running from the ears for a long time, and for three months an ulcer behind one ear, of scrofulous appearance, but without disease of the bone, and was always subject to slight spasms. Three weeks before death he became feverish, with screaming and vomiting, and soon after nearly comatose, with spasms; his bowels were rather loose,

the pupils not dilated, the pulse quick and small. He was in this state when I first saw him; the coma abated, and recurred repeatedly before death.

On dissection, the ventricles of the brain were found much distended with serum. There were two large tubercles, the largest of the size of a hazel-nut, in the medullary substance of the hemispheres. The pia-mater lining the base of the brain was very vascular, and on this vascular membrane, in front of the tuber annulare, were two patches of lymph, the one having the usual diffused form, and as large as a half-crown, and the other somewhat larger, and of precisely similar appearance, except that it was for the most part composed of little tubercular masses, set close together. The substance of the brain, just above this last, was red and softened.

The gradual and imperceptible transition of the matter constituting tubercles, into that which is clearly the effect of inflammation, is, of all the phenomena observable on dissection, that which appears to furnish the most direct evidence of the formation of tubercles by inflammatory action. I have no doubt of being able to form a series of preparations, in which a gradual transition shall be obvious, from the true hepatized condensation of the lung, through the intermediate changes of structure to which Laennec has given the name of gray hepatization, and of tubercular infiltration, to that kind of disorganization which consists for the most part of tubercles; and I am much confirmed in my opinion as to the close analogy of these structures, by finding that the researches of Andral have led him to the belief, that the appearance to which Laennec gave the name of "*Tubercules infiltrés*," is always to be regarded as the effect of a chronic inflammation.*

One important link in this series of preparations must consist of cases of the kind of tubercles called *granular* by Andral, and minutely described by him in the 3d volume of his *Clinique Medicale*, (p. 4. *et seq.*) These have just the external appearance and size of pretty large tubercles, but, when cut into, appear merely nodules of hepatized induration, confined to minute lobules of pulmonary substance. In a case which I attended lately, one lung was found generally studded with tubercles of this description, while at its upper part only there was a pretty large irregular excavation, containing some clots of blood. The patient had died from repeated fits of hæmoptysis. This case illustrates the analogy of this to the more common form of tubercles.

I have seen several fatal cases of adults, in which almost the only morbid appearance found on dissection in the lungs was a deposition of common tubercles, so extensive as to have produced death before they had passed the first stage of their progress, and when their appearance was that called miliary; and in these cases both the external causes and the symptoms of the disease appeared to me to be

decidedly those of inflammation, less acute, indeed, than where the hepatization of the lungs has been the chief appearance on dissection, but nevertheless abundantly well marked.

The most striking instance of this kind, of which I have a full account, is the following.

George Gordon, æt. sixteen, of delicate habit, and somewhat emaciated, was admitted into the Clinical Ward on the 6th of January 1823, complaining of frequent, severe, hollow cough, with copious, rather difficult, puriform expectoration; of pain under the sternum, and difficulty of lying on the right side; and of very short and difficult breathing. His pulse was 140, soft and compressible; his tongue of a florid red, with much thirst; his skin hot and moist; his face flushed, but lips livid, and countenance very anxious.

He described having had, from exposure to cold, one morning before breakfast, a distinct febrile attack, with rigours, five weeks before admission; since which time the cough and shortness of breath, with thirst and heat of skin, especially at night, had continued and increased, particularly during the last few days; he had struggled, nevertheless, to continue his employment as a printer, and had been in the printing-office on the 3d of January, the fourth day before admission.

After being laid in bed, his pulse felt somewhat firmer, and the heat of his skin was 102°; he was bled at the arm, and became faint after 3x had been taken, without any improvement on his breathing being perceptible. A blister was applied, and rose well without any better effect. An expectorant mixture, ether draughts, and the digitalis, with a little wine, were used without benefit. He became delirious on the day after admission; his breathing continuing very quick and short, while his pulse became feebler, and skin cool; and he died on the evening of the 9th.

We found that he had been a patient in the hospital under Dr. Spens, from the 20th October till the 11th November; he had complained, on admission, of slight febrile symptoms, with cough and pain under the sternum, of five days' standing, and ascribed to cold. His pulse was 86. He used a blister and purgatives. On the sixth day after admission, he was reported to have no pain of breast, and very little cough; on the eighth, to be free from complaint; and on the eleventh he was dismissed cured. His friends said, that previous to this time he had not been subject to any pectoral symptom whatever.

In mentioning his case at lecture, the day before his death, I stated, that from his emaciated and enfeebled appearance, from the duration of the symptoms, the peculiar hollow cough, the puriform expectoration, and the clean florid tongue, I had no doubt of his being phthisical, but that the dyspnœa was much more urgent than usual, at that or indeed at any period, during the ordinary course of phthisis; that the history of the case, and the symptoms we saw seemed to indicate, that this depended on an unusual degree of pneumonic

* See Lombard's Thesis, p. 32.

inflammation being *combined* with the changes in the lungs that are peculiar to the first stage of phthisis; and that there was thus an injury done to the texture of the lungs, which must be inevitably and rapidly fatal; much sooner than the ordinary course of phthisis could be expected to be.

On dissection, there was a very slight deposition of lymph on the pleura pulmonalis of both lungs. The lungs themselves appeared externally to be healthy, and crepitated almost universally when cut; but they were studied throughout with an immense number of very small tubercles, exactly corresponding to Laennec's description of "*Tubercules Miliaries*." I had never before seen so very general a deposition of these bodies, in a state so uniform, throughout the lungs. In the upper parts of the lungs only, they had the yellowish colour, opacity, and hardness, of the "*Tubercules Crus*;" and here some of them were nearly as large as peas. Only one was observed in a state of suppuration.

The proper pulmonary substance was of a somewhat redder colour than usual, and more serum than usual ran from it when cut; and in a few places, there was a little condensation or hepatization of it; but this was to a very small extent only.

I consider this case a very important addition to the set of cases adduced in my former paper, in which well marked pneumonic symptoms existed before death, and were never recovered from, and in which tubercles were the chief or only morbid appearances found on dissection.

We know, that on the 11th of November, this lad appeared quite free from complaint, having been kept in the hospital for some days before that time, after the catarrhal symptoms, for which he had come in, had disappeared. His pulse was then natural; and if his breathing had been difficult, or even peculiarly hurried, it must have been remarked. A fortnight after his dismissal he caught fresh cold; the complaint began with distinct rigours, followed by cough; this advanced insidiously for a time, but became attended with pain of breast, thirst, and other febrile symptoms, and then with dyspnœa; at the end of five weeks these symptoms were very urgent; his pulse 140, heat 102; his fever had no hectic exacerbations or remissions; he died before the end of the sixth week; and had his body not been opened, I think the disease could have received no other name than a neglected pneumonia, occurring, no doubt, in a phthisical habit.

On dissection, however, the common unequivocal appearances of inflammation were to a small extent only; but the lungs appeared disorganized to a great extent, by tubercular deposition. These tubercles were so *very numerous*, that I think we can hardly suppose that they could be deposited, or could exist, in the lungs, without materially impeding their functions; they were (with the small exception already noticed) *so exactly in the same state of progress*, that their deposition must have been simultaneous; they were so small that it must

have been recent; and besides, they were of the kind which the most experienced observers regard as the first stage of their formation. To me the influence seems almost irresistible, that they had been formed during the five or six weeks preceding his admission; if so, then we know that their formation had been preceded and attended by the usual symptoms of pulmonary inflammation, occurring in a scrofulous habit, and advancing, probably on that account, more slowly than is usual.

If we reject this opinion, in regard to the origin of the tubercles, and suppose them to have existed in the lungs before the fatal illness commenced, then we must admit that *nothing* appeared in these lungs which could explain the urgent dyspnœa, which was the chief symptom of that illness, and cause of the death of the patient; the degree of hepatization of the lungs, in this case, having been quite trifling, and the serous effusion in the lungs not more than is often seen when the respiration has hardly been affected before death.

It would appear, that tubercles seldom form in the lungs in so great numbers at once, as to be quickly fatal; and to this we must ascribe the rarity of cases of this kind, where there are so many tubercles, without any vomica or ulcers, in writings on phthisis. The case which I have seen described, that comes nearest to the above, in the symptoms and appearances, is one recorded by Bayle,—the second case that he has given at length. A postillion, æt. 24, previously quite healthy, was seized, on the 17th November, with a dry cough. Some days after he had a fall from his horse, and his breast was much hurt. From this time his breathing became short, and the cough was aggravated; these symptoms increased; he became hoarse; his legs became slightly swelled; his appetite continued, but he was unable for his employment. On the 7th December he was taken into the hospital; his breathing was then very short and difficult; he had much cough, with frothy expectoration; his pulse was very small, frequent and irregular; his tongue red, rather dry in the centre, but covered with whitish mucus on the edges; his voice almost gone, and countenance expressive of exhaustion; he died on the ninth, "*dans un accès de suffocation*;" *i. e.* the twenty-second day after the commencement of the cough. He did not appear at all emaciated, and had very slight anasarca.

On dissection, the lungs were not adhering, and appeared healthy externally. When cut into, it is said merely that the substance of both appeared red, and loaded with innumerable whitish semitransparent grains, all about the size of a pin's head, more closely set together in the upper than in the lower lobes. This was the only morbid appearance.

It is obvious that the remarks I have made on the case of Gordon apply equally to this case; but my chief reason for quoting it is, to point out the difficulty which attends the view of the case which is taken by Bayle himself, who, believing that tubercles are altogether

independent of inflammation, suppose these to have existed before even the cough had commenced. Twenty-three days before his death, says he, no medical man, examining this young man, who then enjoyed the most brilliant health, could have suspected that he was consumptive. Yet phthisis already existed in him, and *was already incurable*. Had he not met with the fall, the true phthisical symptoms, depending on the progressive changes of the tubercles already existing, would soon have appeared, and might have been long of running their course. But the fall had given a shock to the lungs, which *deranged their functions, and produced death*. It is needless to animadvert on the vagueness of this phraseology; but it is important to observe, that Bayle had no better account to give of the cause of death in this case. And indeed, it is obvious that if we suppose, with him, that tubercles are never an effect of inflammation, we must suppose, in this case, *first*, that the tubercles caused no symptoms; and, *secondly*, that the cause of death left no marks; that the lungs were loaded with innumerable tubercles at the time when the young man appeared in the enjoyment of the most brilliant health; and that the affection of the lungs which supervened on the injury, and went on increasing, till it was fatal, three weeks thereafter, was merely a disturbance of the functions, not an alteration of the structure of the lungs; the only alteration of structure that was found, having been of the kind which this theory does not allow to be a product of inflammation, and cannot, therefore, refer to the agency of a cause of inflammation.

My belief, on the other hand, not grounded on these cases, but certainly strongly supported by its application to them, is, that an inflammatory action in the lungs, denoted first by the sudden attack of cough on the 17th of November, and afterwards aggravated by the injury of the chest, was the cause of the original deposition of the tubercles, or at least of their rapid progress and increase, within three weeks, from a state in which they were compatible with *the most brilliant health*, to a state in which they so far interfered with the functions of the lungs, as to cause *death by dyspnoea*. Even if we adopt the latter conclusion only, the question, whether the rudiments of the tubercles existed prior to the fatal illness, or whether there only existed the constitutional disposition to their formation, seems to be divested of all practical importance.

Dr. Denis records a striking case of very general tubercular deposition, chiefly found in the first stage at the time of death, in the lungs and bronchial glands, and on the pleura, peritoneum, and omentum, in a young child which had been exposed on the Boulevards of Paris in very cold weather, and was taken into the *Enfants Trouvés*, in a state of extreme depression from cold, seven weeks before death;* and I think no one can read his reflections on

the case, without remarking how much he is embarrassed in drawing his conclusions as to its pathology, by the preconceived notion that tubercles could not proceed from inflammation; and therefore, that the great mass of them there found could not be ascribed to that barbarous exposure.

The affections of the intestines and mesenteric glands, which are so apt to occur in the course of phthisis, appear to me to furnish frequently unequivocal examples of tubercular deposition resulting, I do not say from active inflammation, but at least from increased vascular action and congestion of blood, which may be properly called chronic inflammation.

It has been indeed supposed by Laennec, that the colliquative diarrhoea of phthisis depends in general on the existence of tubercles in the intestines, and of ulcerations formed in these tubercles; and it is certain, that in those patients who are affected with this diarrhoea long before death, these tubercles and ulcers in the intestines, as well as more or less of the tubercular degeneration of the mesenteric glands, are very generally found. But I have had several opportunities of observing, in cases where the colliquative diarrhoea, although well marked, has been of shorter duration before death, that the mucous membrane of the intestines has presented no morbid appearance, except general vascularity; and in other cases there have been only a few tubercles at the most vascular points; from which I conclude that the numerous tubercles and ulcers found in those cases where the diarrhoea has lasted long, are the consequence, not the cause, of that increased vascular action of the part, which produces the diarrhoea. And on examining the mesenteric glands in persons who have died of phthisis after various degrees of the colliquative diarrhoea, or the different mesenteric glands in the same subject, it has appeared to me obvious that the first change which takes place in them, is an increase of size and of vascularity; that the first tubercular deposition occurs at minute points in glands already thus altered, and that it is only by the gradual extension of these deposits, in glands larger and more vascular than natural, that the conversion of the glandular substance into the scrofulous cheesy matter is effected. The first part of these changes in the abdomen, in phthisical persons, is I think generally attended with pain, though not constant, and not always severe.

Another set of cases to which I would beg to refer, as furnishing strong evidence on this subject, consists of those where a disease, which ultimately takes the form, and exhibits the appearance, of tubercles, appears manifestly to have been excited by external injury. I have seen several examples of persons certainly previously healthy and vigorous, who have been subject to shotness of breath, and other pectoral symptoms, only from the time of their receiving such injuries on the chest, as confined them to bed for some days together; and in whom the disease has ultimately turned out to be tubercular deposition, chiefly or solely situated in the immediate neighbourhood of

* Op. Cit. p. 473.

the parts injured. The following case appeared to me the most striking of this kind that I have seen.

A boy, æt. twelve, a year before his death was severely injured on the *lower part of the left side of the chest* by a fall. According to the account of his parents, he had been quite free from any pectoral complaint before that time, but had been confined to bed for some weeks immediately after the injury; had never recovered his strength, and his breathing had always been short, insomuch that he had seldom been out of doors after that time. He died of confluent small-pox, without suffering nearly so much from dyspnœa in the course of that disease as many others do. On dissection the *lower lobe of the left lung* was found adhering to the pleura costalis, and completely condensed, chiefly by red hepatization, but partly by circumscribed tubercles, which were of the yellow colour and cheesy consistence, several of pretty large size, and one of an irregular form nearly as large as a hazel-nut. There were no other tubercles, and no other morbid appearances in either lung.

From the history of this case I think we cannot doubt that the partial condensation of the lungs had resulted, for the most part, from the inflammation that had been excited by the injury. And if we do not regard the tubercles as likewise the effect of the inflammation which had condensed the pulmonary tissue around them, we must make two suppositions, both highly improbable; *first*, that the tubercles had pre-existed in that part of the lung only which is known to be least subject to their deposition; and, *secondly*, that this unlucky portion of the lung, which was already exclusively the seat of tubercles, had been likewise that which alone sustained injury from the fall.

The very great frequency of phthisis in masons, in this country, has been already remarked, and it was stated that, in many of these cases, occurring at a somewhat advanced period of life, less tubercular disease is found on dissection than in younger persons who die of phthisis. Since that time I have met with few cases in which more or less of tubercles were not found; and I have seen several fatal cases, under the age of forty, where there was a very great number of them, in men originally of robust habit of body, and who were free from pectoral complaints up to the age of thirty. If we suppose, as I think we must do, that the phthisis of masons is chiefly to be ascribed to the irritation of the particles of sand inhaled, we may regard the undoubtedly peculiar prevalence of the disease among them as a proof that long continued irritation by foreign bodies may occasion the development of tubercles in many, in whom they would not otherwise have appeared.

The question, whether tubercles can be excited by external injury, and consequent inflammation, is obviously susceptible of illustration from experiment; and the experiments of Cruveilhier on this point, published in the *Nouvelle Bibliotheque Medicale* for September 1826, are so satisfactory as to leave no

doubt on my mind of the correctness of the conclusions above drawn from the different pathological appearances I have described. He injected mercury into the femoral artery of a dog, which was killed some days after this had been done. He then found in the limb thousands of little miliary tubercles, quite regularly formed, and each surrounding a very minute globule of the mercury. He injected, also, by an opening in the windpipe, $\frac{3}{4}$ ij of mercury into the lungs of a dog. The greater part was rejected by coughing: the animal became apparently phthisical, and died emaciated at the end of a month. The lungs were found "*farcis de tubercules isolés et agglomérés, ayant tous les caracteres des tubercules mili-aires.*"

Similar experiments were performed in the Royal Infirmary here, in the beginning of this winter, by my friend Dr. J. P. Kay (author of the paper on the Physiology of Respiration, which was read to this Society, and afterwards published in the *Medical and Surgical Journal* for January last;)[†] and the results of some of these I can now exhibit to the Society.

A small globule of mercury was introduced into the tracheæ of each of the rabbits which were the subjects of the experiments, by small incisions, which soon healed. This produced at first much coughing, which occasionally returned afterwards; but the animals did not appear much incommoded, and took food well; their breathing, however, appearing rather hurried.

The first rabbit was killed eight days after the introduction of the mercury; and it is singular that the appearance of clusters of tubercles was more distinct in this than in any of the others. These lungs are preserved, but the colour of the tubercles does not now contrast with that of the pulmonary substance as before the lungs were put in spirits, and several of the clusters were destroyed in examining them minutely. The appearance of several of the clusters was so exactly that of tubercles in their early stage, that my learned colleague Dr. Monro, and several other friends, gave them that name without hesitation, before they were aware how they were produced; but on cutting into them, each contained in its centre a minute globule of mercury.

It is to be observed farther, that, in several parts of these lungs, there was partial hepatization.

The appearances found in the other rabbits are accurately described by Dr. Kay in the following paper.

"The *second* rabbit, after appearing drowsy and oppressed for a day, was found dead eight days after the operation.

"The upper lobe of the left lung was discovered hepatized in the whole of its anterior border, through which were scattered small granular bodies, of a yellowish colour, each con-

* *Nouv. Bibl. Med.* September 1826.

† *Vide Journal of Foreign Medicine*, Vol. I. p. 481.

taining in its centre an extremely small globule of mercury. They had all the external characters of ordinary tubercles. In the centre of this lobe existed a cavity, containing a soft caseous substance, surrounded by a firm membranous cyst. It was about two and a half lines broad, and three in length. Large globules of mercury escaped from this cavity. In the posterior part of this lobe was an extremely small cyst, containing neither tubercular matter nor mercurial globule.

"The lower lobe of the left lung was also hepatized in the whole of the anterior border, through which numerous small, granular, and apparently tubercular bodies were deposited, containing generally a small globule of quicksilver in their interior. The superior lobe of the right lung was, in some portions, hepatized. A grayish, and apparently tubercular substance was infiltrated into portions of the pulmonary texture. In the anterior part of this lung, existed a large encysted cavity, filled with a soft, yellowish, granular mass, of a caseous consistence, resembling the matter of softened tubercle. The cyst was distinct, apparently well organized, being of firm texture; and the cavity was capable of containing a large pea. In the lower lobe of this lung, tubercular deposition also existed, surrounded by hepatized structure. Several bronchiæ were much dilated, and a similar substance was deposited round their parietes.

"The other rabbits were killed four weeks after the operation.

"*Third Rabbit.*—In the upper lobe of the left lung, a large globule of mercury was found imbedded in a quantity of soft and grayish lymph, surrounded by a distinct cyst. Many globules of mercury were found scattered in the lungs, without either hepatization or deposition of lymph. The lung, however, appeared emphysematous, and some dilated bronchiæ were observed. In the right lung the appearances were similar. The globules generally were found in the thin border of the lung, and appeared to have excited little inflammation there. Some partial condensation, and a few granular bodies resembling tubercles, were observed.

"*Fourth Rabbit.*—Much hepatization had been produced in this lung. Dilated bronchiæ were observed in many portions, and in the extreme thin border of the tissue many globules of mercury existed, unsurrounded by hepatized structure or lymph, but apparently contained in enlarged cells. In the centre of the lung, as before, they were, in some cases, surrounded by lymph, which had the form of tubercles.

"*Fifth Rabbit.*—The general appearances in this lung resembled those described in the others; but, at the extreme thin border of the lung, a row of globules of mercury existed, not simply contained in air-cells, but surrounded each by a very evident thin deposition of lymph."

The tubercular appearances in these lungs (which were always slighter than in the first,) were also much destroyed in examining them, and two small portions only have appeared

worth preserving; the one showing an ulcerated cavity, with a distinct cyst resembling that of a tubercular excavation; the other showing a row of small tubercles (each no doubt containing a little globule,) which have formed along the edge of one lobe, apparently in the cellular substance just beneath the pleura. In this situation the thickness of the lymph surrounding the globules is less than where similar appearances were found in the central parts of the lungs.

I consider the results of these experiments (coinciding as they do with those related above,) as sufficient proofs, that, by the inflammation which is generated by the irritation of foreign substances, depositions of lymph may be excited in the lungs of these animals, which present all the external marks, and which appear, in some instances, to run the usual course, of tubercles.

If we consider it established that masons and others, who are in the habit of inspiring irritating particles, are much more subject than other persons to the deposition of precisely similar matters in their lungs, I think we cannot doubt that the deposition is produced in the same manner; and we know that the matter there deposited produces the ordinary symptoms, and undergoes in general the ordinary changes of phthisis.

It may be said, that if this kind of irritation, acting on the lungs of healthy rabbits, is supposed to produce a deposition of tubercles, resembling those which we distinguish in the human body as scrofulous, we depart from the doctrine generally received among physicians, and illustrated in the former part of this paper, that a peculiar general scrofulous diathesis is much concerned in the production of tubercles in the human body.

I would answer, 1st, That if it be true, as matter of fact, that mechanical irritation of the lungs will produce deposits in the lungs, not differing in appearance from scrofulous tubercles in their early stages, we must not set aside that fact, because it does not accord with our preconceived notions of the pathology of the diseases, in which similar deposits takes place in the living body.

But, 2dly, It was stated already, as the result of the observations of Andral, that the conditions which appear most requisite, in order that inflammation may generate tubercles in the living body, are the *long duration* and *slight intensity* of that inflammation. It is highly probable that the scrofulous diathesis disposes inflammation to terminate by tubercular deposition, simply by giving to it these characters,—keeping it up long, and not permitting it to rise high.

Now it is very easy to conceive, that the continued irritation of a minute globule of mercury imbedding itself in a portion of pulmonary substance, or the repeated irritations of particles of sand inhaled, may furnish just the same condition to the inflammation which they excite,—as is given in other cases by the peculiar strumous diathesis,—rendering the inflammation long continued, but of slight inten-

sity. When this cause is habitually applied for a long time, and during youth, to the human species, as in masons, I believe that very few cases exist, even of constitutions previously strong and healthy, in which more or less of tubercular deposition is not excited.

It is to be observed, that the appearance of tubercles is by no means the only change which was determined in the lungs of these rabbits, by the irritation of the mercury. In each of the cases some portions of the pulmonary substance were found hepatized,—probably parts which had been irritated by larger masses of the mercury than those which caused the deposition in the tubercular form. Such larger masses were in fact found, enclosed in enlarged bronchiæ, in some of these hepatized portions, but in general they appeared to have escaped into the cellular substance close on the pleura.

I would farther observe here, that in asserting that inflammation does in certain circumstances cause deposition of matter, which takes the tubercular form, I do not assert that such products of inflammation must always necessarily run the course that is commonly followed by tubercles. It has been rendered almost certain by Dr. Baron and others, and I have seen various cases illustrating the observation, that the subsequent course of substances, which in their commencement have all the characters of common tubercles in the lungs, may vary remarkably. I have no doubt that the course of tubercles, once deposited, may vary according to the age and constitution of the patient, according to the diseases which may subsequently attack him, and according to the texture in which they have been formed; and on another occasion I hope to be able to show, that the pathology of the earlier stages of diseases, generally regarded as very different from scrofulous affections, does not differ materially from that of scrofulous tubercles deposited in certain circumstances of inflammatory action.

In the mean time, I think the facts stated in this paper, and in the latter part of my former one, may be regarded as sufficient evidence of the proposition, that scrofulous tubercles may be, and often are, deposited in consequence of inflammatory action; and therefore, that as, on the one hand, scrofulous diseases may be in many cases prevented by applying the *tonic regimen* to persons of feeble constitution, but not yet affected with actual disease; so, on the other, they may also be frequently prevented by the early and prudent use of the *antiphlogistic remedies* in those in whom the slight inflammatory complaints so often preceding them have already appeared.

This general conclusion does not differ from that which I believe the greater number of practitioners have been induced to form, from their own practical observation; but it seems to me of great importance to have attained to it by pathological inquiries, founded on fatal cases,—because, until the pathological question as to the frequent dependence of tubercles on inflammation is determined, no observa-

tions on the effect of the antiphlogistic remedies, where tubercular disease is apprehended,—if made on cases that terminate favourably,—can be held to be conclusive on the subject.

From the London Medical Gazette.

MEMOIR ON A NEW METHOD OF
TREATING ARTIFICIAL ANUS. By
BARON DUPUYTREN.*

It is with just sentiments of diffidence that I proceed to speak of artificial anus, a malady at once loathsome and dangerous, which condemns those who have the misfortune to be afflicted with it, to give up the world, burdensome to themselves and others; and which makes them languish in misery, or carries them off from slow and painful marasmus.

Artificial anus has been generally looked upon as incurable; but I trust that, after the details I am about to enter upon, it may hereafter be ranked among those maladies which admit of relief from art, without much difficulty or danger.

It was requisite that I should first endeavour to ascertain the exact anatomical condition of the parts in this affection. In the natural state, the aliments traverse, in a given time, the whole length of the intestinal canal, and undergo, in each of its parts, a series of different changes, as the result of which they furnish to the absorbents the elements of nutrition; after this, the residue passes on towards the anus, and is expelled by actions, which are under the control of volition.

The length of time which they remain, the space which they traverse, the successive elaborations, the absorption of the chyle, and the evacuation of the residuum, constitute a series of necessary conditions indispensable to the regular action of the alimentary canal. Hence it happens that if, in consequence of any disease, these numerous conditions are altered, or even impeded, the digestion becomes disturbed, and more or less diminution of nutrition follows. This is what takes place in preternatural anus—a malady which consists either in an original or accidental opening in the alimentary canal, at a point different from the proper anus, by which opening the aliments, or the feculent matters, are evacuated involuntarily, and before they have been subjected to the necessary changes. The preternatural opening is rarely congenital, but almost always results from wounds, with or without loss of substance; inflammations, abscesses, and particularly from hernia, terminating in the destruction of a portion of the intestine. I mean only to speak of the latter variety—artificial anus.

This condition is by no means so easily produced as might be supposed; and even where life can be preserved only by means of this in-

* *Memoires de l'Academie Royale de Médecine*, 1828.

firmity, nature and art united often fail to overcome the difficulties opposed to its formation. Art fails much oftener than nature, because, in order that it may be produced without danger, certain preparatory steps are required, which are within the power of nature, but beyond the reach of art.

The establishment of an artificial anus, in fact, requires the co-operation of many circumstances. It is necessary that the intestine, at the expense of which the new anus is to be formed, should be placed opposite that part of the abdominal parietes through which the matters are to make their exit; that the intestine should admit of being kept in this situation,—or, still better, that it be fixed in the opening; that a ready communication can be kept up between this aperture and that in the bowels; and, above all, it is necessary that these be capable of forming adhesions to the neighbouring parts,—circumstances, the simultaneous occurrence of which experience has shown to be rare. Once established, the artificial anus presents an opening formed at the expense of the intestine and abdominal parietes, intimately united together. This opening, almost always round, but occasionally irregular, varies in size, from a few lines to an inch or more in diameter, and is surrounded by radiating folds of the skin plaited upon itself. The border presents throughout a cicatrix, uniting the skin of the belly to the mucous membrane of the bowel. Round the aperture exists that union between the intestine and abdominal parietes without which the preternatural anus could not be formed.

These adhesions are the product of inflammation, and always commence in the serous surfaces of the intestine and abdominal cavity; and thence extend to the other textures, soon reaching the skin and the mucous membrane. In hernia, these adhesions precede the destruction of the parts, and thus prevent the escape of the intestinal contents into the abdomen. In wounds, again, they do not take place till after the division of the intestine; and this is the reason why these are so frequently fatal. Their extent varies; it is from half a line to a line, in most cases—but in others it is several lines, and sometimes, though rarely, extends through half an inch. The medium of union is a substance which successively passes from a glutinous state to a cellular, and, at length, to a fibrous texture. Arrived at this last stage, it is sufficiently strong to resist effectually most of the causes tending to separate the bowel from the walls of the abdomen. But as these adhesions never extend very far along the intestines, it results that a sort of *cul de sac* is formed, the opening of which looks towards the belly, and the bottom of which corresponds to the skin. Into this cavity the abdominal viscera are protruded, in some individuals, so as to produce herniæ, which obstruct, or even alter the position of the artificial anus.

The opening of the anus is almost always occupied by some part of the internal membrane of the bowel, irregularly puckered, and

of a more or less deep red colour. Not unfrequently, protrusions of the bowel take place, the mucous membrane becoming irritated and inflamed. This eversion generally occurs at the upper end of the intestine, sometimes at the lower, and occasionally at both at once; but always forms a curved line, owing to the shape and resistance of the mesentery. Its length varies, from one to fifteen or more inches, and it may be easily conceived how much it must add to the pain and inconvenience.

Between the opening of the skin and the bottom of the artificial anus, there is a kind of funnel-shaped cavity, which Scarpa has well described. This is formed at the expense of the various parts which inflammation and the contact of the alimentary matters have brought to a state identical with that of mucous membrane. The skin forms its border, the intestine its base. Its length, direction, form, and dimensions, vary infinitely, and have very great influence on the cure of the artificial anus. The greater the length and capacity of this funnel, the greater, in general, the tendency on the part of nature to cure the infirmity, or to second the efforts of art in effecting this object.

It is in the bottom of this cavity that the most remarkable and important dispositions of the artificial anus exist. There the orifices of the two extremities of the intestine, and the partition which separates them, are to be found. Of these openings, one belongs to the part of the intestine leading from the stomach, and, in consequence of the feculent and alimentary matters always passing through it, it is the larger and freer of the two. The other orifice belongs to the inferior extremity of the intestine; and as it does not receive any, or, at all events, but very little of the above matters, it is generally narrow, puckered up, and difficult to find.

Beyond these two orifices are the two extremities of the intestine, of which they are the terminations. These extremities, which are villous, and covered with mucus internally, and moistened with serous secretions externally, retire into the abdomen, sometimes crossing and sometimes parallel, but most frequently separating from each other at a greater or less angle; and at length they become more and more curved, till they are lost among the general convolutions of the bowels.

Between the two orifices, placed across, is a projecting angle, more or less marked. This projection, noticed and described by Saviard and Morand, is produced by the juxta-position and union of the sides of the intestine. Formed by the part of the bowel which the mortification or the wound has spared, on the side next the mesentery, this projection juts forwards, nearer to or farther from the skin, according as the intestine has suffered a greater or less loss of substance, and undergone more or less considerable change in its situation. It is small, and scarcely to be seen in the depth of the funnel, when the intestine has only just been pierced by a wound or eschar, and when

it runs along the posterior surface of the parietes of the abdomen in the natural direction of its curve. But it is very great, and comes out to the level of the skin, when the whole circumference of the intestine has been destroyed, and when, in consequence of this, the two extremities meet at a sharp angle, and, *à fortiori*, when they are parallel. In the former case, there exists, between the two orifices of the bowel, a kind of gutter, which may still direct the matters from the upper one towards the lower; and this, therefore, is the kind of preternatural anus most easily cured. In the latter case, there is no vestige of this gutter; and the projecting *buttress* of which we speak, placed between the two ends of the intestine, forms a barrier which the intestinal contents can neither break down nor get round;—this is the kind of anus most difficult to cure.

This projection does not divide the bottom of the funnel into two equal parts; or even if this be the case at first, it does not long continue so. In fact, thrown aside by the passing current of matters from the upper portion of the bowel, it becomes applied to the lower orifice, acting the part of a valve, and concealing it: hence the difficulty often experienced in finding the lower opening.

This buttress, examined from the cavity of the intestine, has the form of a crescent, the angles of which presenting from the concavity towards the convexity of the bowel, lose themselves insensibly on the inside of the gut, or on the borders of the artificial anus. Examined from within the belly, it is seen to unfold itself, and the two equal parts of which it is composed separate and receive the mesentery between them. This division of the buttress at its base is the result of its mechanism: it is not formed of one single wall except at its sharp edge; at every other point it consists of two sides, having a triangular interval between them, which becomes larger in proportion as they separate from each other on entering the abdomen.

It results from this, that the openings of the two ends are separated by a double partition, the surfaces of which towards the belly are smooth, and free from any adhesion; so that, in order to pass from one of these openings to the other through the intervening partition, it is necessary to traverse the peritoneal cavity. From this arises the difficulty and the danger of attempting to establish a communication between the two portions of the canal, by attacking the projection which separates them.

The buttress and double partition are not fixed so firmly but that they can advance and recede: attached to the mesentery, they follow to a certain extent the movements communicated to them by that ligament. The distribution of the mesentery in artificial anus, though less important than that of the intestine itself, yet merits consideration. Stretching from the anterior part of the vertebral column to the concave part of the intestinal convolutions, it has, in the natural state, no greater

extent than between those two points; and although extensible, it is always more or less dragged when the intestine leaves its natural situation, and is protruded from the belly, as in most cases of hernia and penetrating wounds of the abdomen, with protrusion of the bowels. Compelled to follow the gut which is displaced, the mesentery forms a kind of cord from the vertebral column to the part of the bowel most distant from it. This cord is necessarily tense, and inclines the body forward; thus preventing the power of keeping it upright, and still more of throwing it back. This is particularly observed in cases of hernia, which are adherent. In consequence of this distribution of the parts, the projection or buttress which has been described, as well as the intestine itself, is constantly pulled inwards by the mesentery, with a force proportioned to the degree of tension in this membrane. Hence we easily perceive the influence which the position and movements of the body must have on the cure of this malady. This tension, however, is not free from danger, as I have known it sufficient, in two cases, to destroy the adhesions which united the extremities of the bowel to the parietes of the abdomen, and thus to produce fatal laceration and effusion into the peritoneum.

This action of the mesentery on the intestine does not cease even when the artificial anus has been cured—it is continued long after, and gives rise to the following remarkable phenomenon. Several individuals cured of artificial anus without operation, having re-entered the Hôtel Dieu after several years, and having died of diseases unconnected with this, I examined the parts, and what was my astonishment when, in place of finding the intestine fixed to the inner surface of the abdominal parietes, I found it free and unattached! I might have suspected some mistake, had not the identity of the individuals been perfectly established, and had I not found a fibrous cord extending from the intestine to the part of the abdominal parietes corresponding to the artificial anus. Thus the efforts of nature were not limited to closing up the preternatural opening; they had even separated the intestine from the parietes of the belly—they had restored its natural curve and mobility, by elongating the cellular substance in the form of a cord; and the smallness of this about its middle justifies the conjecture that its laceration would at once have removed the last trace of the derangement which had preceded, accompanied, and followed the formation of the artificial anus.

Nor are these the only changes which take place. The upper extremity of the bowel, excited by the presence and passage of the intestinal contents, acquires increased activity and size; a change in which the mesentery and lymphatic glands participate. The lower portion, on the other hand, ceasing to perform its functions, becomes gradually attenuated, till at length one part of the canal resembles that of an adult, and the other that of a new born infant. Nevertheless, the lower

end does not become obliterated, nor is it even altogether empty—it is filled to a certain extent with the usual intestinal secretions, which, from some remains of the natural functions of the part, are converted into a white mass, of a soft consistence and albuminous appearance, and which may remain, without undergoing decomposition or causing uneasiness, for months, or years, till it is either voided by a natural effort, or washed out with injections.

The consequences produced by artificial anus are these: in the natural state, the intestine free, and floating in the abdomen, though attached to the mesentery, describes a series of uniform curves, along which the contents pass without difficulty; but no sooner is an artificial anus established, than this becomes altered. A portion of intestine directed towards a particular point of the abdominal parietes forms a triangle, the base of which is towards the mesentery, and the sides of which are formed by the upper and lower extremities of the bowel.

The mobility, another condition necessary to the changes of situation, volume, form, and, above all, to the peristaltic motions, by means of which the alimentary matters are propelled:—this mobility is changed, through a greater or less extent, into an absolute fixture, caused by the new adhesions. This fixed portion becomes a *point d'appui* for the efforts of the canal, so that the intestinal contents are constantly directed towards it; and hence results a real acceleration in their progress from the stomach downwards. The space traversed by the aliments is diminished—the period of their detention is abridged—their digestion is rendered incomplete—nutrition is impaired, and the evacuation of the bowels is removed from the control of volition. Every animal has an alimentary canal, the length of which is in proportion to the nature of its food, and each portion of the canal exerts upon the aliment which passes along it an influence different alike from that which precedes and that which follows it; and hence the artificial anus, by diminishing the length of the canal, lessens the degree of elaboration which the food undergoes, and thus impairs digestion—the more as the preternatural opening is nearer the stomach. Thus we see voided from an artificial anus, sometimes matters which are digested; sometimes such as are but half so; and at others, we see the food pass unchanged. In some individuals, the nutrition is not remarkably diminished; in others, it falls off rapidly; and in those who have the artificial anus very near the stomach—as for example, at the commencement of the small intestine—the strength diminishes, the body wastes, and the patient dies of inanition, after a time which varies according to circumstances.

With regard to the involuntary evacuation of the intestinal contents, the opening is not surrounded by any muscular apparatus capable of acting upon it at will; and the aperture is, therefore, always open to the matters which are constantly arriving. Besides, even if there were the necessary muscular arrangement, the

contents of the bowels, deprived of a reservoir where they can become united, retained, and formed, as in the great intestine, would constantly require to be voided. There is thus a constant flow of mucous, biliary, alimentary, or feculent matters, according to the state of digestion and the situation of the opening; and hence the person of the patient is affected with an offensive smell, and the parts are liable to excoriations, erysipelas, and intolerable itching, which renders existence a continual torture. All the contrivances to obviate these evils, do so but very imperfectly; and compression, so as to retain the matters within the bowels, often gives rise to such mischief as to render its abandonment absolutely necessary. It is evident that the buttress and partition which separate the two extremities of the intestine, are, by their greater or less projection into the artificial anus, the causes which facilitate or oppose the cure. How are these obstacles to be overcome? Can we push back towards the belly the parts which form them? Might we divide them by incision—by ligature—or by a slow and graduated section?

In the first part of this memoir I have detailed the nature of the obstacles that prevent the restoration of the intestinal canal; nevertheless, these obstacles, although considerable, are not insurmountable; nature and art have more than once succeeded in overcoming them. The loss of substance of the intestine is certainly irremediable, but the dilatation of its cavity, and the extension of the parietes, may supply, in certain cases, that loss, in a manner more or less complete. The adhesion formed by the intestine to the abdominal parietes, may become less intimate; it may be relaxed in such a manner that the two ends of the gut may be placed in a more favourable situation for the passage of the fecal matters. The projection which separates the two extremities may become diminished by the dragging of the mesentery, and by the efforts made by the fæces to pass from the superior portion of the gut to the inferior. A liberal diet, as recommended by Louis; the action of purgatives, as advised by other writers; the introduction of pieces of charpie, gradually increased in size, as practised by Desault, may have the effect of enlarging the communication between the two extremities of the gut; and these, together with position or compression, and many other means proposed, have no doubt occasionally effected cures. But under what circumstances have they succeeded? It is necessary to distinguish these from cases in which such attempts must always fail; and where, consequently, it becomes necessary to adopt more efficacious means.

Almost all preternatural anuses which consist of simple perforations of a point in the circumference of the intestine, whether attended by hernia or not, are curable; they are, in fact, only fistulæ, behind which the gut is always perfect, not having suffered either loss of substance, contraction, or material change of direction.

The same means of cure will also succeed very frequently in those cases of artificial anus in which a third, or even half the circumference of the gut has been destroyed for the length of a few lines, or even an inch, although accompanied by an inversion of the intestine; but when the loss of substance embraces more than two-thirds or three-fourths of the circumference, and also includes a greater length, the cure becomes proportionally difficult; for then, from the contraction in the caliber of the gut, the buttress and partition become so prominent as to present formidable obstacles to the passage of the fecal matters from the upper to the lower portion of the intestine. The result of the cases that have occurred to me, as well as of those which I have collected from different authors, amounting each to a considerable number, is, that the proportion of artificial anuses susceptible of cure, are to those which obstinately resist every plan of treatment as 3 to 1; that is, two-thirds are cured by the ordinary methods, and the remainder require a more efficacious plan of treatment. The difficulties then that oppose themselves to the cure are the loss of substance and contraction of the gut; the adhesion of its extremities to the parietes of the abdomen; the changes produced in its direction, and in its mobility; but especially the projection and double partition placed between the two extremities.

The loss of substance cannot be repaired; it can only be supplied as we have before said, and we have already shown the limits within which this can be effected. Can the adhesions which at first formed the safety of the patient, be destroyed without producing a recurrence of the original danger?—and even if they were destroyed, the loss of substance remaining, our embarrassment would not be much lessened. It is true, in imitation of Rhamdor, the two ends may be placed within each other, maintained in that situation by suture, and then replaced in the abdomen; but the danger of this method is obvious, and the example given by this surgeon will scarcely be followed by any reasonable man.

It is necessary then, to respect this salutary adhesion, and, therefore, it only remains to attack the partition and buttress. It would seem, at first sight, that the simple section of these parts, either by the scissors or some other cutting instrument, would be sufficient to re-establish the communication between the two ends of the gut; and it must be confessed that such would be the case if the two sides of the projection adhered together; but a moment's reflection will show that this operation must produce almost immediate death, by the effusion of fecal matter into the cavity of the abdomen. It seems more prudent, therefore, to displace the buttress and the partition by pushing them into the cavity by pressure from without inwards, so as to imitate, in some degree, the effects of the dragging of the mesentery. If these trials should not succeed, they cannot, in my opinion, produce any inconvenience. Influ-

enced by this idea, I constructed an instrument which I shall not describe, since it failed in the only trial I made with it, in consequence of the impossibility of regulating its action with any certainty; thereby risking the sudden rupture of the adhesions, and consequent effusion. Being obliged to renounce this method, it only remained either to perforate or divide the projection. The idea of perforation first presented itself to my mind, but it appeared difficult to execute this without producing the same mischief that a common cutting instrument would cause—that of effusion into the peritoneum.

The two ends of the intestine which form the artificial anus, are covered on all sides by the peritoneum, and this membrane forms an uninterrupted cavity round them. This circumstance, which forms an insurmountable obstacle to an immediate division, or perforation, affords the very means by which the double partition separating the intestines may be divided without opening into the cavity.

One of the most remarkable properties of serous membrane is to form adhesions when inflamed, and when in contact; if, then, an inflammation could be excited between the two surfaces of the intestines in contact, and covered with the peritoneum, capable of producing adhesions, I conceived that I should afterwards be able to perforate and divide the parietes of these intestines, and establish a communication between the two extremities without risk of an effusion into the abdomen. But the difficulty was to find a mode of producing this preliminary adhesion of the intestines.

My first idea was to traverse the partition by a needle, which would rather pierce than divide the parts, and which would convey a thread to fill up the void that had been made; this thread, after having excited inflammation around it, might be increased in thickness, and afterwards replaced by a skein, increasing in size from day to day; so that, after some time, it might be large enough to destroy the partition between the two extremities of the gut entirely. Then their cavities would become re-united, and means might be adopted, without inconvenience, to prevent the passage of the feces by the artificial anus, and to compel them to follow their natural course. A consideration of what is often found to take place during the passage of foreign bodies, especially of pins and needles that traverse the parietes of the abdomen and the intestines, tended to confirm this idea; for the passage of these bodies through the different parts is always preceded by the *adhesive inflammation*, which is, in fact, the preservation of the patient. These suggestions were the result of observation only. I wished to strengthen them by direct experiment upon living animals. With this view, I traversed the intestinal canal of several dogs, with needles armed with threads, which I left in the wounds, putting back the intestines into the abdomen. No effusion took place in any instance; the wounds and the threads, after some time, were found sur-

rounded by adhesive inflammation; the ligatures were either voided by stool, or taken away by gently pulling both ends at the same time; the openings made by the needles, and those in the parietes of the intestine, were always found closed, adhesion having taken place between the peritoneal coat of the divided or punctured intestine, and the peritoneum of the neighbouring parts. A still more decisive experiment, attended with the same result, was made by forming an artificial anus in a dog.

My experiments had arrived at this point when, in May 1813, a man named Aucler was admitted into the Hôtel Dieu, 36 years of age, who had laboured under strangulated hernia for five days, the consequence of which was the formation of an artificial anus, the intestine having been found in a state of gangrene. Six weeks elapsed, and nature appeared to afford no prospect of a cure. At first pressure was tried, but this produced symptoms so severe, as to compel me to abandon its use. The patient, however, continuing to urge me to employ other means to relieve his miserable condition, whatever might be the result, an attentive examination showed me that the two extremities of the gut were perfectly on a level, and that their orifices were only separated by a very projecting buttress and partition. After considering the best method of perforating this partition, I determined to pass a needle across it, from as high up as possible in the cavity of the upper end of the bowel; its point was then received into the cavity of the lower end, and thus brought out: the thread with which the needle was armed was left in. The operation was short, and not very painful; it produced no unpleasant symptom. Some days afterwards a skein was carried, by means of the thread, into the opening made in the partition. Flatus began from that time to be passed by the natural anus: the size of the skein was increased at each dressing; and eight days after colicky pains were felt in the abdomen, and some fæces were passed by the anus. Encouraged by this, the size of the skein was increased to such a point as one day to produce a laceration of the buttress; this produced no ill effect, but still stercoraceous matter continued to pass from the artificial anus. Considering that those parts of the partition situated above the opening made by the needle, and enlarged by the skein, might adhere together, and might be divided with as little danger as the parts situated below, this part was divided, half a line at a time, by means of a pair of blunt pointed scissors, directed upon the fore finger; this was done at intervals of three or four days: these incisions, very cautiously made, and which never passed the limits of the adhesions formed, enlarged the communication so much that all the fæces soon passed by the natural anus. Compression was then used upon the artificial anus, which would, most probably, in time have closed the opening; but the man, wishing to hasten the cure, urged me to make a fresh attempt, and I had

the weakness to yield to his entreaties. Some irregular portions situated round the aperture were tied, and then excised; I afterwards carried the division of the partition higher than it had yet been done, and in a few hours the patient was seized with acute peritonitis, which proved fatal, notwithstanding every means adopted to arrest its course. I apprehended that this inflammation might have been produced by the effusion of fæcal matter in the abdomen, but at a public examination of the body, no such effusion was found; there was no solution of continuity by which such an accident could have happened, and the cavity contained merely a quantity of purulent serosity, and albuminous flocculi, the ordinary products of acute peritonitis. The communication between the two extremities of the gut was re-established for the space of about two inches. The ends, before separated, had but one wall and one cavity; along the whole length of which, before and behind, there was a raphe, produced by the cicatrix of the section of the partition; and every thing announced, that had not this unfortunate accident intervened, this artificial anus would have been cured.*

Chagrined at the unfortunate result of this case, I again reviewed the question; and I was confirmed in my previous opinion, that the idea of establishing a communication between the two ends of the intestine, by destroying the partition that separates them, was the only mode that promised any chance of success, and that the only defect was in the means hitherto employed. It was evident to me that, though the passage of the needle, in the above case, had produced no accident, yet by penetrating the parts before adhesion had been effected, the communication it established between the intestine and peritoneum might, in certain cases, produce an effusion in the cavity; besides it appeared difficult, if not impossible, to carry the needle and thread to such a height as to enable them to open a communication between the two ends of the gut, sufficiently large to re-establish completely the course of the fæces in every instance; finally, (and this was the most conclusive reason of all,) the needles and thread could only produce adhesion of the parts, if they were in contact with each other; and if, instead of being parallel and touching each other, the sides of the intestine should be separated at the spot where they were penetrated, a perforation without adhesion would be produced, and the result of the operation would be merely to establish a very dangerous

* In a note, M. Dupuytren observes, that the method he had just detailed, and which he thought peculiar to himself, had been practised in Germany by Dr. Schmalkalden, as detailed by Dr. Koreff; and in America, by Dr. Physick, as related by his son-in-law, Dr. J. Sing Dorsey, in his *Elements of Surgery*, published at Philadelphia in 1813. M. Dupuytren quotes the whole passage.

communication between the cavity of the intestine and that of the peritoneum: these considerations induced me to renounce this method of treatment. It became necessary, therefore, to devise a method of keeping the parts in contact previously to dividing them, and which would not effect their division until adhesion had taken place between them; and recollecting the phenomena that present themselves during the passing of pins and needles, &c., I perceived that, following the example afforded in those instances, it was necessary first to produce inflammation, then adhesion, and lastly the division of the parts. At length, after many trials, upon the dead body as well as upon living animals, I believed that I had discovered the instrument which I had sought for. This instrument is composed of three pieces; two branches and a screw. Each branch is about six or seven inches in length, and one which may be called the male, because it is destined to be received into the other, has a blade four inches long, three lines broad, and half a line thick at its edge, which is undulated and terminated by a spheroid button. At the union of the blade with the handle, is a mortise some lines in extent; behind this mortise is a handle, one, two, or more inches, having another mortise running nearly the whole of its length, about three or four lines broad. The female branch is not quite so long as the former; it is composed at one of its extremities of two blades of the same length, breadth, and thickness, as the male blade; between these two blades is a sort of gutter or sheath, destined to receive the other blade. At one of the ends of this blade is a cavity to receive the button of the other. At the junction of the blade with the handle, there is a moving pivot, which is to be received into the mortise of the other branch; the handle is terminated by a hole destined to receive the screw.

The third part of the instrument is a screw of several threads, an inch and a half long, terminated by a plate of an oval form; this screw is to be placed in the mortise of the male branch of the instrument, and fixed in the female branch; its use is to separate or close, at pleasure, the two blades of the instrument. This instrument I named an *Enterotome*. Its application is easily understood: two branches, which may be separated or united at pleasure, provided with blades, very blunt, and with a waving edge, are put into motion by means of a screw passing across their handle. All that these blades enclose is seized, and retained by them by means of their form, as well as by the introduction of the one into the other. The pressure which they exercise upon the parts they embrace, has the effect, at first, of placing them in contact, and it may afterwards be carried to the extent of destroying their vitality, but not of dividing them immediately, the edges being too blunt to effect this. This instrument has not since undergone any alteration, but has been applied subsequently to every case of artificial anus upon which it has been necessary to operate.

However, before I employed it upon man, I applied it to other living animals, and its effects surpassed my expectations. Upon each occasion it succeeded in dividing the parts in six or eight days; and in every case where serous membrane was confined within the branches of the instrument, these membranes, and the parts they invested, were united together from the second or third day, and consequently long before the solution of continuity, which does not happen till the seventh or eighth day. This adhesion, which extended on each side of the branches of the instrument throughout their whole length, though easily broken down at first, became so firm in five or six days as to resist considerable force; at a later period it became cellular, and afforded all the solidity of a natural attachment. What may appear astonishing is, that the action of the enterotome was never attended with severe pain, and the inflammation was always confined to the immediate vicinity of the parts laid hold of by it. This was the more remarkable, since its mode of action did not appear so simple as might have been imagined. It did not produce solution of continuity, like a cutting instrument, that is, without any loss of substance; on the contrary, it caused a real mortification of the parts embraced by it, and the loss of substance is caused by the formation of slough, which, when it separates, is always between the blades of the instrument. It was not long before I had occasion to employ it on a patient named Menage, and having proved successful, I afterwards used it in many other instances.

I will now detail the first case in which the enterotome was employed.

— Menage, æt. 26 years, had suffered from his infancy from an inguinal hernia on the right side, which had never been attended to, and became strangulated on the 2d January, 1815. At the termination of the sixth day, after vain attempts made at reduction, the operation was performed. The intestine was in a state of mortification, and the feces passed by the wound. In spite of every means, an artificial anus became established, by which all the evacuations passed. They presented themselves at the aperture generally about an hour and a half after a meal, and they were passed, not in the order of their introduction into the stomach, or in that of their digestibility, but rather in relation to the quantity of nutritive matter contained in them; those containing but little nutriment passing out first. The man's appetite became enormous, notwithstanding which he daily lost flesh and strength. About eight weeks after the operation, he experienced violent colic, followed by evacuations per anum, and these were renewed at long intervals.

Such had been the state of Menage for a year, at the end of which time he was admitted into the Hôtel Dieu. The artificial anus was about half an inch in diameter, it was bordered by irregular tumours, arising from the puffing up of the mucous membrane of intestine, behind which appeared, whenever the pa-

tient made the least exertion, a hernia which raised it up, and carried it outwards, giving rise, sometimes, to an invagination of the intestine; the neighbouring skin was extremely irritated; the man suffered great pain, and the stench he emitted was excessive. The patient was eager to have some attempt made to cure this infirmity. My first step, after appeasing the irritation of the skin, was to determine the position of the two ends of the intestine, which were drawn downwards by the hernia situated behind the artificial anus. At length I discovered the direction of the extremities, as well as of the buttress and partition; and immediately I introduced separately the blades of the enterotome, to the greatest possible height, into each of these ends; and after having fastened them together, I closed them moderately; the patient experienced no pain; they were tightened on the following day, and some colicky pains ensued. In a few days, the blades of the enterotome became a little moveable; about the sixth day there were abundant evacuations by stool; the instrument fell off on the eighth day; the blades contained nothing but a membranous band, in which all the tunics of the two adhering parietes of the gut were cognizable. The length of this membrane, which was as thin and dry as parchment, was twenty lines, by two in breadth; this was the exact measure of the depth to which the instrument had been conveyed, and consequently that of the loss of substance which the partition of the intestine had undergone. From this time all the fæces passed by the natural anus, and their escape by the artificial one could be prevented by pressure; however, although this was a good deal narrowed, it did not heal up: to obtain this desirable end, various means were employed—compression, bandaging, sticking-plaster in strips, the use of nitrate of silver, &c. Seeing the obstinacy of this opening, now little more than a line in diameter, I excised the edges, and brought them together by means of the twisted suture; and afterwards employed a particular instrument for making pressure. At length, after four months' labour, I had the pleasure of presenting this patient, entirely cured, to the Faculty of Medicine.

In order to apply the instrument, it is necessary, first, to seek for both the orifices of the intestine, and to determine exactly the direction of the canal. This is the most difficult part of the operation. The upper orifice is, indeed, easily found; but, to discover the lower, the finger, or a soft probe, must be employed often, for several days. These points being ascertained, and the patient placed upon his back, one of the blades of the enterotome is directed, by means of the index finger, towards one of the orifices of the gut; it is placed within this orifice, which it is made to penetrate, according to the nature of the case, one, two, or three inches in depth. This done this blade is given to an assistant, and the second blade is introduced, with the same precautions, and to the same height, into the other extremity of the intestine; the two blades

are then brought together, and articulated in the manner of a pair of forceps, by putting the tenon of the one into the mortise of the other. It is sufficient at first, to take hold of the intestine, and to bring the blades of the instrument together in the same way as when cutting with a pair of scissors. The action of the enterotome being intended to be slow and gradual, it can only be kept up by mechanical means. This is done by the screw. The power of the blades is such as to destroy the life of the parts embraced by them. The pressure ought to be so managed as to destroy the life of the part from the first day; it is by so doing that the pain and inflammation are prevented. This pressure is to be increased every other day, by giving the screw a turn or two. It might appear, at first sight, that an instrument carried to such a depth in the abdomen, and pressure made to such an extent as to destroy the parietes of the intestines, would produce colic, vomiting, inflammation, and other severe accidents—but such has not proved to be the case. Indeed, few of those to whom the instrument has been applied, have experienced any but very slight pain: a very small number have suffered from colics and vomiting; the fæcal matters have preserved their course; the inflammation has been confined to the portions laid hold of by the instrument, and has not been communicated either to neighbouring or distant parts. After a few days, the instrument becomes a little moveable: this mobility increases day by day, until it falls off without any pulling, pain, or bleeding; and this happens always between the seventh and tenth days. When it has fallen out, the blades are found nearly closed, containing within them a membrane similar to that above described. Maceration in water shows the nature of this membrane. It is by this loss of substance that the buttress and partition separating the two ends of the gut, are destroyed, and the proper course of the fæces thereby re-established. These evacuations sometimes precede the falling off of the instrument, and at first are white and albuminous, consisting of matters contained in the lower part of the intestine only; and afterwards, they become stercoraceous, being transmitted from the upper portion. At first they are numerous and liquid, with pain, gripings, and straining; they gradually become more solid, and less frequent; the appetite becomes moderate, and the strength and flesh are restored. The most difficult part of the cure remains—that is, to obliterate the false external opening. Many weeks are requisite to accomplish this.

The following case proves, that the above plan is equally applicable to those instances of artificial anus resulting from wounds attended with loss of substance of the intestine.

Louis Tubert, aged 42, was admitted into the Hôtel Dieu, in March 1824, with an artificial anus. This man was weak of intellect, small of stature, with a yellowish muddy complexion, extremely thin and feeble. Eighteen years prior to the date of his admission, he had produced a rupture at the ring of the left

side, in consequence of a violent exertion. The volume of the tumour increased insensibly; so that, at the end of fifteen years, it had acquired the size of an infant's head, and was, in a great measure, irreducible.

Believing himself to be an object of ridicule on account of this infirmity, Tubert conceived that he should be able to rid himself of it by an operation. Without communicating his plan to any one, he made a large incision in his scrotum, opened the hernial sac, and gave issue to a knot of intestine eighteen inches long. He then became alarmed, and sent for a surgeon, who, with some difficulty, reduced the gut, and the patient got well; but the hernia remained—for, considering a bandage as merely a palliative cure, he refused to wear one. He still continued to cherish the notion that he could cure himself by an operation; and brooding over this for about three years, at length, during the absence of his wife, on the 22d February, 1824, he made another incision into the scrotum, opened the hernial sac, and, bolder than on the former occasion, he laid hold of the intestine and cut off a portion. The pain, bleeding, and the issue of fecal matter, however, alarmed him, and he once more sent for his surgeon, who enlarged the opening in the scrotum, found the two extremities of the divided gut, and reunited them by several points of suture. These failed in uniting the intestine, but they produced inflammation of its extremities, which united them to the lips of the wound, and thus an artificial anus became formed. The part removed was a portion, two inches and a half in length, of the small intestine; it did not form a complete cylinder, but was interrupted at two points, one for the extent of about half an inch at its extremity, and the other about the centre, rather larger on its mesenteric side. On his admission into the Hôtel Dieu, there was found, on the left side of the patient's scrotum, a long tumour, extending from the ring to the bottom of the scrotum; it was hard, shining, partly reducible, and exhibited, at its lower and anterior part, a wound of a vivid red colour, formed below by the scrotum, above by the two ends of the intestine, reverted and twisted upon each other so as to make several turns. They were placed side by side: that on the right gave issue to some fluid feces, mixed with undigested matters, such as pieces of carrot or other vegetables; this was continual and involuntary. The other end of the intestine was retraced, and did not discharge any thing. The patient was in a filthy state, rendered worse by the habit so common to maniacs—that of handling the excrementitious matter. He suffered also from colics, as well as from a fixed pain and tension in the left iliac region. After the lapse of a few days, the two reverted ends of the intestine were reduced, and a compressive bandage applied to the artificial anus; enemata were then administered, and a regulated diet established, but pressure could not be borne; it was tried several times, but always occasioned symptoms rendering it necessary to abandon its use.

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The man continuing to waste, I determined to seek for the disposition and connexions of the two extremities of the gut. I found that the upper, or *stomach* end, was situated at the bottom of the scrotum, where it formed inextricable circumvolutions, and that the *rectal* end led directly to the ring. This situation of the upper end of the gut, caused me to hesitate as to any attempt at a radical cure; but, at length, the urgent entreaties of the patient, who had heard that similar infirmities had been cured, induced me to make the attempt—especially since the patient's mind became so intent upon this plan, that I dreaded his making an attempt at a third operation himself. I accordingly proceeded on the 31st May, in presence of MM. Larrey, Aumont, and Sanson, to introduce the blades of the instrument, separately, into each extremity of the gut, passing them in as deeply as possible. The upper blade could only be carried to a depth of from two and a half inches to three, and in this situation I was obliged to close and fix the instrument. On the first day there was no pain; the next day there was an œdematous swelling, and some redness at the edge of the artificial anus, but still there was no pain. On the sixth and seventh days, slight colics were felt; the eighth day the instrument fell off, and the two extremities of the intestine formed but one canal. From this time, clysters were given every day; foetid gas passed per anum; but the feces still made their way by the artificial anus, and therefore the patient continued still to become thinner and weaker. After the lapse of a fortnight, Tubert conceived that he had passed feces by the natural anus, and the volume of the tumour diminished. Some time after this, pains in the belly began to be felt: their violence at first threatened to exhaust the man's remaining strength;—however, the evacuations became established in the proper channel, they acquired regularity, and the patient's strength and flesh in some measure were restored. The size of the tumour gradually decreased, but still some fecal matter passed by the artificial anus: to arrest this entirely, I applied an apparatus for the purpose of holding in contact the lips of the wound. This compression is composed of two segments of a circle, very open, of equal size, a few inches long, and some lines only in breadth, placed parallel to each other, each surmounted by a shank of an inch and a half high; these shanks are united by a cross-piece fixed to one, and moveable upon the other, which receives it in a groove with which it is pierced. Beneath this cross-piece is a screw, which rests upon one of the shanks and moves upon the other; and the movements of which to the right and left, produce, as required, either the separation or approximation of the compressing arches. These being padded with linen, or stuffed, are separated—the skin in the vicinity of the artificial anus is raised up, and the fold which it forms is insinuated between the arches; a slight motion given to the screw, from right to left, bring these segments of the circle together, and thus the artificial anus be-

comes so compressed that nothing can pass through it. When this compressor was applied to Tubert, it happened as I expected; nearly all the faecal matter took its natural course; the little that still continued to ooze out was suppressed by an increased constriction of the instrument, and then, for the first time, a smile was seen upon the patient's countenance. However, the instrument sometimes got loose, and at others produced excoriations of the parts, and then the faeces began again to flow from the wound; and this occurring several times, gentle and constant pressure with a bandage was substituted. From the period that the excrement passed by the natural channel, the patient rapidly recovered his flesh and strength, so that his appearance alone was sufficient to show whether there had been any discharge from the artificial anus or not. A triangular flap of skin, situate at the upper part of the artificial anus, resulting from the irregular incision made by the patient, seemed well adapted to close what remained of the aperture; this flap was therefore touched with solution of lunar caustic, as well as the edges of the opening, and it was then applied and maintained in this position by the assistance of a proper bandage. The flap united, as was expected, and completely closed the opening—thus perfecting the cure in something less than five months.

I could greatly multiply the examples of cure by the method above related, but the detail of a number of cases would add nothing to what I have already said; it will be more useful to give the general result of the operations I have performed according to the method above described. This method is not theoretical, neither are the cases of artificial anus so uncommon as to render the relation of this plan a matter of indifference. Every year a certain number of such cases is admitted into the hospitals of the capital, and doubtless many hundreds exist throughout Europe. The result, then, of the facts collected in my practice, and as well as of those communicated to me, or made public by different medical men, is, that forty-one operations for artificial anus have been performed by means of the enterotome; that is, twenty-one by myself, and twenty by other practitioners; among whom I am proud to reckon M. Lallemant, Professor at Montpellier, one of my most distinguished pupils. Three-fourths of these operations were rendered necessary in consequence of gangrene from strangulated hernia; the other fourth, in consequence of wounds, with loss of substance of the intestine, to a greater or less extent. Of these forty-one operations three only have been followed by death; one from a presumed effusion of faecal matter into the abdomen, one from indigestion, and one from peritonitis, caused no doubt by continuity of surface between the parts interested in the operation and the peritoneum. Of the thirty-eight remaining patients, by far the greater portion experienced no serious symptoms; some few were affected with nausea, vomiting, or pains in the belly, but these

were remedied by simple means. The whole number were not equally well cured. Nine have preserved, in spite of every thing that could be done, fistulous openings, more or less in extent, which have obliged them constantly to wear a bandage. *Twenty-nine have been radically cured in the space of from two to six months.* Thus, in considering the danger of the operation, it has caused death in one case out of fourteen only; and if the death by indigestion, which ought not reasonably to be attributed to the operation, be excluded, the proportion of the deaths is reduced to one in twenty.

[This memoir, although but just published, was read four years ago at the Academy of Sciences, and a considerable number of operations with the enterotome have since been performed in several countries of Europe; the result of which does not sensibly affect the conclusions here detailed.]

From the London Medical Gazette.

ESSAY ON SYPHILIS. By JOHN BACOT, lately Surgeon to the First Regiment of Guards.

The appearance of another publication on syphilis may demand some apology. It will, perhaps, be urged that the subject is exhausted—that pamphlets upon particular points of doctrine, and elaborate treatises, comprehending both the theory and practice in every form of the disease, have become so common, that nothing remains to be gathered by the industry of the most attentive gleaner in this field of inquiry.

In answer to such assertions, it may be suggested that the mass of information thus admitted to exist, is scattered through a vast variety of publications; that few writers are agreed as to the theory of the disease, or even respecting the practice to be adopted; that the late doctrine of the multiplicity of venereal poisons has materially contributed to unsettle the opinions of practitioners; and that the whole question remains at present in a state of doubt and uncertainty, which must be extremely perplexing to the younger members of the profession.

The essays now offered to the public are the result of much reading, and of an extensive experience in this class of complaints; and it is hoped that they may materially tend to save much of the valuable time of the student, by presenting to him, in one view, the result of the opinions of most of the principal writers on syphilis; by enabling him to form some conclusions as to the justice of the peculiar views entertained by Mr. Carmichael and others, as well as of those opinions respecting the non-mercurial treatment, advocated so freely in this country of late years.

It may be proper to observe, that the style adopted is necessarily plain and colloquial, having originally been delivered in the form of lectures; this will also account for the want

of references to the various quotations, which, however, are faithfully given, and may be depended upon; finally, it is hoped that if these essays should be found to be free from any practical imperfections, and calculated, by the information they contain, to supply the student with a full and sufficient guide to the treatment of the varied symptoms of the disease, the absence of all pretensions to fine writing will be pardoned.

South Audley-Street, 1st June, 1828.

It has often been remarked, that those who have been long engaged in any particular study or pursuit, are too much inclined to claim for the object of their choice, a greater degree of consequence than it really merits; and to expatiate upon its importance with a warmth of zeal that appears ridiculous in the estimation of the impartial or indifferent spectator. It must be my endeavour to avoid this common error; but that I consider the subject of syphilis as one of peculiar interest, demanding a greater share of attention than is usually bestowed upon it in the course of a surgical education, is sufficiently evident by my venturing once more before the public in the character of an author.

The veteran practitioner, fully aware of the extent and difficulty of the undertaking, may, perhaps, rather be disposed to censure the temerity, than applaud the courage, of one who, fully sensible of the difficulties that surround the subject, has voluntarily imposed upon himself so arduous a task; whilst the tyro in surgery, who has, perhaps, imbibed the notion that the cure of syphilis is comprised in one simple precept (the administration of mercury,) may be disposed to think that I am engaged in a most unnecessary and uncalled for pursuit. If, however, I should be so fortunate as to satisfy those whose experience and knowledge entitle them to judge of the merits or defects of my performance, it will, I trust, be no difficult matter to obviate the objections of the junior part of the profession. Never, surely, since syphilis became an object of professional inquiry, has there been a period when some positive and determinate doctrines were more imperatively called for, than the time in which we live. In proof of this assertion, I may ask the practitioner to take a survey of the present state of opinion, both public and professional, relative to this disease: scarcely can we find any two surgeons agreed as to the most simple points, either of its theory or practice. If we turn to the recorded opinions of modern writers, the confusion is not lessened: one teaches us that there are three or four venereal diseases; another, that scarcely any thing but pseudo-syphilis is now to be met with: a third goes a step farther, and asserts, that there is not now, and never has been, such a disease in existence, and that for upwards of three centuries we have been prescribing for a phantom of our imaginations; a fourth would fain persuade us that we have only been mistaken in our means of cure, and that mercury is not necessary for any form or symptom of the dis-

ease; whilst, of those who adhere to the old remedy, some are advocates for a thorough saturation of the constitution at all events, and at all hazards; whilst others think, that as an alternative, mercury may be admitted into the number of our auxiliaries, but that it is not to be relied upon alone: in short, if any one should seek for illustration of the often quoted apothegm of the father of physic, that "art is long, and life is short," he cannot choose any more appropriate than the venereal disease, which, after the lapse of more than 350 years, appears to be less understood now than at any period since it became an object of inquiry and interest.

But if the theoretical views entertained respecting this class of diseases have so materially changed, it is no less certain that the practice has undergone, within the last 20 years, a most extraordinary revolution: that good may not eventually result from the conflict of opinions, it is far from my intention to deny, or even to doubt; but it is only very recently, indeed, that the enthusiasm of novelty appears to have expended itself, and that something like a rational and systematic line of conduct begins to be followed.

In adverting to the increased, and still increasing frequency of some of the secondary forms of the venereal disease, I must, however, be allowed to explain, that although these are unquestionably the result of the recent inquiries that have been made into the natural history of the disease; yet those who set the inquiries on foot are not responsible for the evils that have ensued. Those inquiries were commenced in the true spirit of philosophical research, and if they have been too implicitly or generally acted upon, the blame must attach to those who adopted the conclusions without due discrimination. Having once suffered their faith in the powers of mercury to be shaken, they lapsed into the opposite extreme, and became infidels at once with respect to the necessity of its employment. Such, indeed, is the nature of mankind; and it is not only in the science of medicine that we may look for an illustration of this truth.

If every field yielded fruit in proportion to the labour bestowed upon it, little, indeed, would remain to be done with respect to the venereal disease. A writer of the last century has told us, that in his day upwards of 400 Treatises had been written on this disease from the period of its first invasion, and I verily believe that the number has been more than doubled since. But when we come to look over this list, and to examine the share of merit belonging to each individual author, we shall find, that when we have rejected the dreams of superstition, the plagiarisms of some authors, and the perverse attachment to system of others, that the number of standard works will be very much reduced, and the stock of our actual knowledge brought within a very narrow compass indeed.

I shall not consume any more time by prefatory remarks, but proceed at once to explain the course I intend to pursue in treating this

subject. I shall first devote one or two essays to the remote history of the disease, and then take the modern doctrines into consideration: having disposed of this question, and stated my own opinions, I shall commence with the symptoms of gonorrhœa, and then go regularly through the primary and secondary symptoms of syphilis itself, together with the treatment best adapted to each form of the complaint. The name by which a disease is designated is not, perhaps, a matter of much importance; but it may be as well to observe, that I shall generally make use of the word syphilis, as denoting the primary affection; and apply to the constitutional symptoms the term of secondary syphilis; for although I may occasionally speak of the venereal disease, or lues venerea, for the sake of varying the expression, still this latter phrase appears to be too vague and general: the etymology of the word syphilis is indeed doubtful, and, after all, not very obvious; yet still it is pretty generally understood, and is liable to no misconception.

In examining into the history of syphilis, two questions have particularly attracted the attention of authors: the first relates to the antiquity of the disease—the second, to its origin. It may, perhaps, be thought by some, that this discussion is superfluous, and that it can have no other result than that of affording an opportunity of heaping quotation upon quotation, and obtaining for me the reputation of a little reading, at the expense of a great deal of time; but I trust that it will be admitted, upon reflection, that this inquiry is one of absolute necessity, tending to complete the character of the liberal-minded and well-informed practitioner, to whom no species of knowledge should be wanting—who should never be content until he is enabled to give a satisfactory solution to all suggestions or doubts that may arise in his mind upon this or any other subject on which he may be engaged. It is by means of this knowledge that he will be enabled to obviate objections, and to surmount difficulties; or to avoid the imputation of plagiarism, by fancying he is suggesting something new, when, in fact, he is only relating or reviving what has been said or done a thousand times before.

The belief that the venereal disease was known long before the period usually assigned for its invasion, has of late been revived by a modern writer of great experience in the practical part of his subject, and whose labours have latterly made a great impression on this country. The view that this writer has taken of syphilitic complaints has rendered this belief almost a matter of necessity to him, since it smooths many of the difficulties, and explains most of the anomalies, that would otherwise encumber his path; it is, therefore, on this account also, more incumbent upon us to notice the historical part of the subject, and to give a glance at the evidence which is afforded to us by the ancient writers in support of this side of the question.

Of this evidence there are two kinds; that which we derive from the medical writers of

antiquity, and that which is to be found in the works of the poets and philosophers of the same period; for I can scarcely suppose it to be necessary to do more than to allude to the opinion that has been broached more than once, that the diseases recorded in the Bible as having affected David and Job, as well as that which is the object of some of the legal ordinances of Moses, were nothing less than lues venerea. Never, certainly, was there an assumption made upon more feeble grounds: it may have afforded some scope for critics and commentators to exercise their ingenuity, and to display their learning, but nothing can be elicited from these meagre and scattered passages that might not be as well applied to many other diseases, or that ought to arrest the attention of the candid inquirer for a moment.

Among the Pagan authorities we find, from certain passages in Hippocrates, Pliny, Celsus, &c., but more especially the latter, that ulcers, both on the male and female parts of generation, were not uncommonly met with; and Celsus, in particular, gives us many directions for their cure. But before I proceed to quote one or two of these passages, I must observe that there is no allusion whatever, in either of those writers, as to the diseases they mention being *solely* or *constantly* the product of impure connexion between the sexes; still less do we find it asserted that their cure was attended with any particular difficulty, or that any after consequences resulted from them. Thus presenting us with a strong and marked line of distinction between these complaints and the venereal disease of modern times.

Hippocrates, in several separate portions of his writings, and particularly when describing the diseases of the female, mentions ulcers of the womb and of the pudenda, warts, swellings of the groin, &c. and he directs them to be cured by the most simple applications. On examining the writings of Celsus, we shall be much struck with the very precise and clear account which he has left us of several affections of the parts of generation; and here I cannot again help remarking, how impossible it would have been for this elegant and acute writer to have omitted noticing the sequelæ of the diseases, had any such existed in his days. Neither ought we to be surprised, considering the debaucheries, the luxurious modes of living, and other concurrent causes which tend to produce diseases in other parts of the dermoid system, that the parts of generation should have been occasionally liable, among the profligate and luxurious of those days, to breaches of surface, to eruptive diseases, and to inflammation and its consequences. In the 6th book of Celsus is to be found an excellent description of a phymosis, and the method of treating it; directions are also given as to the mode of curing the sores that shall be found underneath the prepuce, when the glans is denuded. Several distinct species of ulceration are detailed. He distinguishes tubercles or *φυματα*, from *φρυμιον* or verrucula. Two kinds of cancer are also mentioned, to

one of which he gives the name of phagedena; and he also speaks of rhagades or serpigenous ulcerations in the neighbourhood of the perinæum, and of condylomata or tumours about the anus, which he ascribes to the action of previous inflammation. I shall beg in this place to give you a translation of a passage from this author, which is no bad specimen of the surgery of the time in which he wrote, and which negatively at least may be considered as a pretty strong confirmation of the opinion I have ventured to pronounce, as to the total ignorance of this writer of any after consequences arising from these ulcerations, which he otherwise could not have failed to allude to in this place:—

“Therefore if, in consequence of inflammation, the penis becomes swollen, and the prepuce cannot be drawn over the glans, it is to be fomented with warm water; but when the glans cannot be denuded, a syringe must be inserted between it and the prepuce, and the parts washed out: if then the prepuce gives way, the cure will be more expeditious. If, however, the swelling prevents this, a poultice of lentils, or horehound, or olive leaves, boiled in wine, to each of which a little honey may be added, may be applied, and the penis must be again bound up against the belly, a precaution necessary to be taken in every mode of treatment of that part; and the patient must also be abstemious, and content himself with water only to quench his thirst. The next day the same means of fomentation, &c. are to be repeated in the same manner, and then a little effort may be made to draw back the prepuce; if this cannot be done, it may be slightly opened with a scalpel, the discharge consequent upon which will diminish the swelling of the part, and the skin will be drawn back more easily. In whichever way this end is accomplished, ulcers will be found either in the inner part of the prepuce or on the glans, or even beyond it, on the penis itself, which are either clean or dry, or moist and purulent.”

The different methods adapted to the treatment of these various ulcers is then detailed; but in every variety the healing of the ulcer is evidently the completion of the cure, for neither in his chapter on diseases of the skin, nor in that in which he describes ulcers of the throat and nose, do we find the slightest insinuation of such symptoms being found in connexion with ulcerations of the sexual organs. The story told by the younger Pliny, in the 24th epistle of the 6th book, is scarcely deserving of being related at length: it evidently alludes to the destruction of the penis, and it was followed by the death of the sufferer, though his death was not immediately caused by the disease. This story adds no direct strength to our argument, it is true; but it may be observed, that the relation would have afforded an opportunity for the writer to have descanted upon the disease and its consequences, had he heard of it, either in degree or kind, as we are accustomed to see it now. I might have extended this account by drawing

your attention to a passage or two in Galen bearing upon this point; but the remarks I have already made upon what Hippocrates has said, apply with equal force to his commentator.

The evidences of the antiquity of the venereal disease which have been culled from the writings of the Pagan philosophers and poets, will next demand a little of our attention. The list of these authorities is certainly formidable, both in point of number, as well as from the reputation of the authors. We find included in this list the names of Herodotus, Tacitus, Suetonius; and still later, Eusebius, the ecclesiastical historian; and Palladius, the bishop of Hellinopolis. Among the poets, Martial, Juvenal, Horace, and Ausonius, have each afforded some expressions which have been eagerly laid hold of by the supporters of this doctrine. After having enumerated this long list of great authorities, I need surely no longer insist upon the necessity of being acquainted with this branch of the subject; and how little should we be prepared to encounter an adversary armed with these learned and imposing names, unless we were in possession of the facts upon which the belief of the antiquity of syphilis is founded; but when once acquainted with them, very little explanation or argument will, I imagine, be necessary to point out the fallacy of the doctrine. For this purpose, I shall first mention what Herodotus relates concerning the spoliation of the temple of Venus Urania by the Scythians, when they invaded Palestine, on which account, says the historian, their descendants were afflicted with a disease which is called in the Latin tongue “*morbis famineus*,” and which the best commentators suppose to have been really a gonorrhœa, or flow of semen, in the strict sense of the word, by which their testicles became wasted, and, in fact, they lost both the powers as well as the appearance of men. So that it is clear, if this mean any thing at all, at least it has no reference to the question at issue. Suetonius, in speaking of the Emperor Augustus, says, “*Corpore ipsum fuisse maculoso, dispersis per pectus et alvum, genitivis notis in modum et ordinem ac numerum stellarum cælestis Ursæ, sed et callis quibusdam ex prurigne corporis, assidueque et vehemens strigilis usu plurifarium concretis, ad impetiginis formam.*”

The passage from the annals of Tacitus is still less to the purpose, for we are simply informed that the Emperor Tiberius, a man infamous for his debauchery, had, in his old age, a bald head, an ulcerated face, and was completely worn out and bent double. I have already alluded to the story told by Palladius, who informs us that a certain person named Hieron, much addicted to intemperance of all kinds, whilst at Alexandria, fell into the snares of a female performer at the theatre, with whom having sinned, he was visited by the divine wrath with an anthrax on the glans penis, which terminated in the loss of all the parts of generation; after which it appears that he re-

covered, and became a miracle of penitence and piety.

The case related by Eusebius is that of a man who had, in the secret parts of his body, an abscess and a fistulous ulcer, which proved to be incurable, breeding an infinite quantity of worms, and of a most fetid and intolerable odour. Here the precise part affected is not even designated: the phrase is "*in mediis occultiorum corporis partium locis*;" and may as well apply to a fistula in the perinæum, or in the scrotum, as to any disease else. No general bodily affection is hinted at, nor are we told that the complaint originated in any improper conduct upon the part of the patient. It will surely not be necessary to pursue this phantom any farther; whoever wishes to consider the arguments which have been drawn from the Roman poets, may consult the second Satire of Juvenal, the 37th Ode of the first book of Horace, the sixth book of Lucretius, and the first book of the Epigrams of Martial;—in these passages he will find abundant evidence of local disease, but not a word that can be construed into any similarity between those affections and the lamentable consequences attending the invasion of syphilis; consequences which might have afforded the finest scope to the satirists and the moral writers of antiquity, and which, in times comparatively modern, as Dr. Friend has justly remarked, has not been alluded to in the writings of either Dante or Boccaccio—who were not a whit more likely than their predecessors to have suffered so fertile a subject to have escaped them.

I may next take a cursory view of what the Arabians have added to our stock of knowledge, relative to the antiquity of this disease; and in doing so, it is necessary to recollect that the medical writers of that extraordinary people were large borrowers from the Greek authors, and were also well acquainted with many formidable cutaneous diseases, particularly the leprosy and elephantiasis. On another account, also, the Arabian writers demand our attention, since from them the employment of mercurial preparations was unquestionably derived; the application of which afterwards, to the cure of the venereal disease, was, in all probability, the result of analogy, since their utility in many cutaneous diseases had been long recognized by the practitioners of that nation. But, before I make any further mention of the Arabians, it will be proper, in order to keep up a connected chain of evidence, to mention a few of the later Greek authors, with the dates of their respective works; showing that from them no additional arguments for the antiquity of syphilis, can be fairly adduced. Of these, I shall merely mention Aribasius, Citius, Paulus Egineta, and Actuarius. The first of these writers does little more than copy what was already known upon the subject of ulcerations, and other affections of the genitals; nor does Etius, though much more copious, add any thing to our stock of information. Paul of Egina talks of the excision of warts, under

certain circumstances; he directs them to be tied, or destroyed by the cautery; and many precise rules are laid down for the treatment of ulcers of the parts of generation, of rhagades, condylomata, and their varieties, under five or six different appellations;—but still the great distinction remains: there is no allusion to subsequent disease as deducible from these local affections—no hint that the constitution might participate in the mischief. Four centuries later, Actuarius flourished at Constantinople. He has been supposed, by some historians, to have been bred up originally in the schools of the Arabians; at least, it is very certain that he borrowed as liberally from them as from the Greeks, and yet we shall search his writings in vain for any new or more precise information with respect to these affections; and I am induced to mention this author in particular, because he forms a fair link of connexion between the Grecian and Arabian authorities, and proves, not only by what he has said, but by his having omitted to say more than is to be found in the writings of his predecessors, that no new symptom, nor additional feature of aggravation, belonged to these complaints in the time in which he lived, than had been remarked for centuries before him.

When we speak of the authority of the Arabian writers, it will be necessary to recollect that the chief learning of that nation was originally derived from the Greeks. Alexandria was taken by the Saracens in the seventh century; and though we are told of the destruction of the famous library, by command of the Caliph Omar, there is good reason for believing that the schools of physic were still kept up in that city, even in the succeeding century, by Christian teachers. It was not until the year 767 that Bagdad was built; from which period, the seat of Arabic learning seems to have been transferred there; yet, even then, most of their philosophy appears to be borrowed from the Greeks. I have considered it necessary to enter into this explanation, lest it should be thought that I have not dwelt at sufficient length upon the writers of this nation: there are, in truth, but few of them who do not mention ulcers, warts, fistulæ, and other affections of the parts of generation; yet, excepting in the adoption of new names, there is little else to arrest our attention; for instance, in the writings of the Arabians, we first read of the Bothar, the Alsophate, the Moram, and Ignis Persicus, though the precise meaning of some of these appellations is by no means obvious; indeed many of them appear to have been synonymous terms, whilst others have been adopted from their supposed resemblance to the effects of fire, as, for example, the Ignis Persicus, "*qui dicitur ulcus carbonosum et generatur ex sanguine ferventi cum cholera mixto*;" just as the Formica was so designated, because the pain it produced was compared to the bite of an ant. Rhazes, to be sure, mentions an ulcer of the penis, which, according to him, was produced in a very strange manner, viz. by the "*accensio*

nem mulieris supra virum;"—and Avicenna gives us reason to believe that the leprosy was sometimes communicated by connexion between the sexes; a circumstance by no means to be wondered at, considering the undoubtedly contagious nature of that disease; but he does not insinuate that there was any thing new or remarkable in this, or that the symptoms differed from those usually met with in leprosy patients. He mentions, indeed, an ulcer of the penis, and heat of urine, as symptoms by no means unusual in the progress of that disease, though it is to be remarked that, some ages before his time, Citiis, in speaking of the contagious nature of leprosy, warns his patient to avoid coition; nay, he declares it to be very unsafe to go near a leprosy patient.

I have now brought down my history to the writers of the middle ages, and have already said enough, I trust, to convince the sceptical that there is no really well-founded reason for believing that any disease, generally affecting the constitution, or tending to the destruction of the patient, was known to the Greeks, Romans, or Arabians, as the direct consequence of connexion between the sexes; but I must now claim a little farther indulgence, in order to clear up the difficulties which the zeal of Mr. Beckett, a very staunch partizan of the antiquity of syphilis, has thrown in our path; for although Astruc has, I think, very successfully combated his arguments, and overthrown his authorities, yet, as Mr. Carmichael has, in his late publication, again brought that author into notice, and appears to give implicit credit to his assertions, it is incumbent upon me to examine a little into his pretensions; and here I cannot refrain from quoting a passage from the work of the learned and acute historian of physic, Dr. Friend, who, speaking of the first invasion of syphilis, says, "It may be proper to observe, that in the earliest appearance of this distemper, as well as since, there were many who, not being used to think, or reason, any farther than as the ancients showed them the way, took a great deal of pains to prove that the disease was known both to the Greeks and Arabians, though but imperfectly described, and represented under the names of the different kinds of leprosy, exulcerations, and other cutaneous affections. And here we have instances how the words of old authors may be wrested and perverted to serve the present purpose, and support a favourite opinion; for their method of arguing was to quote by scraps—to pick out one symptom out of one author, another out of a second, and so on, till they at last dressed up such a disease as the ancients had not the least notion of." Again, the same author very pertinently remarks, in answers to those who believed syphilis to be no other than leprosy, under a new name, that the leprosy was then a common disease, and could not want such an extraordinary phenomenon in the heavens as is related by some authors to account for the first appearance of syphilis. But to return to Mr. Beckett:—this gentleman wrote as lately as

the year 1718, and his papers are to be found in the 30th and 31st volumes of the Philosophical Transactions. The chief authorities he adduces in support of his opinions are the following:—the first is to be found in the writings of John Arden, an Englishman, who, it appears, lived at Newark in the year 1349, and afterwards removed to London.

Among other stories relating to this subject, it would appear that he mentions a disease called *arsura*, which consisted of an internal heat, with an excoriation of the urethra; but this is only a repetition of what is to be found in the Arabian writers. Beckett might have added, that he also relates cases of abscesses and schirrous tumours, that form in the penis, but not one word does he say either as to their origin or consequences; nor can we doubt that, in the first named disease, he speaks of gonorrhœa. Mr. Beckett's second argument is drawn from the ordinances of the Bishop of Winchester's Stews in Southwark, where the disease of brenning, or burning, is recognized as the product of impure connexion, and many regulations are detailed to prevent the spreading of the disease. This argument, however, I need not enlarge upon, since it is of pretty general notoriety, and has received ample confirmation from several authors of that age, particularly in a work of Dr. Boord's, published in 1546; from a tract by Dr. Bulleyn, in 1562; and also from a manuscript by one John Bayle, in the possession of Mr. Beckett himself. Another source of argument might also be derived from the statute published by Joan, Queen of the two Sicilies, and Countess of Provence, in the year 1347, the fourth article of which is to the following purport. The queen commands, that, on each Saturday, "the bayless of the brothel, and a barber deputed by the consuls, do visit all the strumpets who shall be lodged in the brothel; and if any one be found who has contracted any disease by fornication, such women shall be separated and lodged apart, in order to prevent the communication of disease to the young men." It is proper, however, to add, that there are some doubts as to the authenticity of this document. So far Mr. Beckett successfully proves that gonorrhœa was a common disease long before the siege of Naples; but that fact has not been denied, and is distinct from the question at issue. He next proceeds to relate some cases, in which the leprosy was communicated by intercourse between the sexes—a truth, the possibility of which no one could deny; but as leprosy was well known in those days, acknowledged universally to be contagious—and, moreover, as no new symptoms are recorded, and no astonishment is expressed by the relaters of these cases, as to such an occurrence, we may fairly conclude that this argument is worth little or nothing.

Mr. Beckett next quotes Theodoric, originally a Franciscan friar, and afterwards Bishop of Cervia, who wrote in the twelfth century. This author is remarkable on several accounts: first, as describing the same disease, the *arsura*, as arising from impure connexion with a leprosy

woman; and secondly, as having been the first who introduced the use of mercurial preparations into practice. The effect of mercurial inunction upon the mouth seems to have been well known to him; and this knowledge he plainly appears to have derived from the Arabians, among whom several formulæ for the preparation of these remedies are to be met with, and which they applied to the cure of many cutaneous diseases. Theodoric is copied largely by our countrymen, Gibbertus Anglicanus, and John of Gaddesden; the latter of whom recommends the following extraordinary mode of cure to the female patient, who is directed to leap backwards down stairs. Such are the principal facts adduced by Mr. Beckett in his first paper. Two years afterwards, he published a second; in which he brings forward two additional testimonies in support of his former opinions. The one is a manuscript in Lincoln College, Oxford; wherein it is asserted that Thomas Gascoigne, Doctor of Theology, the author of the manuscript, was acquainted with several persons who had died of the putrefaction of their genitals, and of their whole body, in consequence of illicit connexion; adding, that John of Gaunt died of this same disease; although, from the context it would appear plainly that no particular disease was alluded to, in his instance at least, but that the death of that prince was owing to "*frequentationem mulierum, magnus enim fornicator fuit.*" The other testimony is that of John Ardern, spoken of above; to which there is, therefore, no occasion to revert. It is strange, however, that Mr. Beckett, zealous as he was, should have overlooked one or two authorities, at least equally strong, or stronger, than any he has produced; but as it is my intention not to conceal any thing, I shall point out these authorities, though I conceive they admit of the same explanation which has been given to the previous quotations. The passages I allude to are found in Lanfranc, Gordonius, and in Gulielmus de Saliceto: the two latter, indeed, only repeat what the former has said, but that is, at least, a proof that the subject matter was believed and recognized in their days. The former of these writers, who lived in the middle of the thirteenth century, speaks of a bubo, which, he says, may arise from a corruption in the the penis of a man in consequence of lying with a foul woman, or from other causes; and the reason he gives for it is curious enough:—he says, that the corruption is multiplied, and retained in the yard, "*unde non potest natura mundificare virgam aut locum, primo propter strictam viam illius loci, unde redet et regurgitat materia ad locum inguinum propter habilitatem loci illius ad recipiendum superfluitatem qualibet; et propter affinitatem quam habent hæc loca ad virgam.*" Gordonius, whose work appeared in 1305, acknowledges abscesses and ulcers in the penis as the consequence of lying with a woman whose womb is foul, and full of virulent sanies. To these descriptions Lanfranc adds, that whoever wishes to preserve himself from corruption, when he has had con-

nexion with a female suspected of foulness, (*immunditia*) should wash the penis with vinegar and water. Now these three authorities appear, upon the first glance, to give strength to Mr. Beckett's arguments, but, in fact, they do no such thing; they prove, I think, in the strongest manner that negative evidence can do, that beyond a gonorrhœal discharge, and ulcers or pustules on the parts of generation, no other evil consequences were to be dreaded from impure connexion, and we shall soon see how strong and marked the contrast becomes; not by imperceptible shades and gradations—not by one author adding one symptom, and a successive writer another—but that *all at once*, towards the conclusion of the fifteenth century, the public become the victims of a train of symptoms altogether unusual and undescribed; rebellious to every mode of treatment then in use—not attacking the poor only—not confined to those whose circumstances and situation in life exposed them to more than the ordinary chances of disease, and deprived of the ordinary means of cure—but exhibiting all its rage, and exerting its baneful powers, over princes, cardinals, nobles, of both sexes; thus proving, not only the severity, but the extensive progress of this hitherto unknown scourge. Surely then, I may be allowed to say, that if there is any single historical fact that can be said to be proved, it is that of the origin of syphilis being referable to the latter years of the fifteenth century; for I cannot understand otherwise, why, at that precise period, we all at once hear of ulcers on the parts of generation in both sexes, followed speedily by excruciating nocturnal pains, by corroding ulcers over the whole body, by affections of the throat and nose, and very frequently by death; when not one word, that can be construed into any similar affection, is to be met with distinctly stated in any writer before that period. I think we have no right to impute so much dulness to our forefathers; they offer no parallel instance of any such gross error in mere matters of fact. Whatever their faults as theorists may have been, as careful observers of what was passing under their eyes every day, so gross a mistake could not have occurred to them; and had such a dogma as the novelty of a really old and well-known disease been started by any interested or ignorant writer of that age, we cannot but believe that it would have met with an instantaneous refutation and denial. I here, then, beg leave to declare my belief that the venereal disease was not known to the writers of antiquity; and, secondly, that, in the course of our research, it has appeared very clearly that a disease resembling gonorrhœa had been prevalent in Europe several centuries earlier, and most probably had been known from the remotest periods of history. But of this, more hereafter.

Having now disposed of this first part of my inquiry, I come to consider the origin of syphilis; in other words, whether it is a disease imported from the West Indies or not? This belief has been so generally adopted, that an

attempt to controvert it may, perhaps, startle the majority of my readers; nevertheless, it seems to me to be an opinion formed too hastily, directly at variance with historical evidence, and acquiesced in, probably, more on account of the apparently useless nature of the inquiry, than from any intrinsic force in the arguments by which it is supported. It may not, indeed, be a matter of much moment to the practitioner, to ascertain whether the common account of the origin of syphilis be true or not; but as a matter of literary research, it is surely deserving of some moments of our attention; and we surely need not refuse to occupy ourselves for a short time in an inquiry which Sydenham thought worthy of his consideration, and which has attracted the notice of Swediaur, Sprengel, and other eminent practitioners. Among those who have doubted the commonly received account of the birth-place of the venereal disease, I might mention the name of the late Mr. Pearson, from whom, indeed, my scepticism upon this point was originally derived, and who has recorded his doubts in his Treatise on "the Effects of various Articles of the Materia Medica in the Cure of Lues Venerea."

To return to my subject; I shall not rely much upon what Sydenham has urged on this part of my argument, since he does not enter deeply into the inquiry, but proceed at once to mention the works of Dr. Sanchez Riberio, of Hensler, Sprengel, and Swediaur, with the authorities they have adduced: Dr. Sanchez published his opinions in two separate periods, his first work appearing in 1765; to this, Dr. Robertson, who, though not a medical man, may be supposed to be a competent judge of an historical fact, is not disposed to attach much importance, but he afterwards says, having seen the second edition of Sanchez's book, "it contains several additional facts in confirmation of his opinion, (that is, that lues was not imported from America) which is supported with such plausible arguments as render it a subject of inquiry well deserving the attention of learned physicians. Dr. Hensler's work was published at Altona, in the years 1783 and 1789; and from these authors may be collected a train of facts and reasonings founded upon them that are well worthy of our consideration. In the first place, they call to our recollection that Columbus returned from his first voyage of discovery in the month of March 1493, to the port of Palos in the Mediterranean sea; (they ought to have added that he first put into the Tagus, and remained at Lisbon for five days;) from Palos he went to Barcelona, where Ferdinand and Isabella then held their court: he was accompanied by some of his crew, and six Indians whom he had brought with him from the island of Hispaniola: the remainder of the ship's company continued at Palos or Seville, and we do not hear that they communicated any disorder in either of those places: how then can we reconcile these dates with what Baptist Fulgocius has related; who asserts, that two years before king Charles's invasion of Italy, that is, in 1492, a new disorder

broke out, for which the physicians knew no remedy? In France it obtained the name of the Neapolitan disease; whilst at Naples it was called the French disease. The testimonies of De Isla and Oviedo, though they both affirm the West Indian origin of syphilis, appear to be deserving of little credit; (indeed the authority of Gonzalvez de Oviedo is worth nothing, for he affirms that the disorder was conveyed into Italy by Cordova's fleet, which, however, did not arrive at Messina until 1495, and consequently not till two years after the disease had existed there;) but in truth both these authorities sink into insignificance when contrasted with the silence of Peter Martyr, who was physician to the King of Spain, and actually at Barcelona when Columbus made his appearance there after his first voyage, and where he remained until nearly the end of the same year, and yet he (Peter Martyr) does not say one word as to the importation of this disease in any of his writings; but this is not all, for the same author, in a letter addressed to Arius Lusitanus, the Greek professor at Salamanca, and which letter is dated in the year 1488, that is, five years before the return of Columbus from America, has the following decisive passage. "In peculiarem nostram tempestatis morbum, qui appellatione Hispanâ bubarum dicitur, (ab Italis, Morbus Gallicus, Medicorum Elephantiasin alii, alii aliter appellant) incidisse precipitem, libero me scribes pede. Lugubri autem elego calamitatem ærumnasque gemis tuas, articulorum impedimentum, internodiorum hebitudinem, juctuarium omnium dolores esse proclamas, ulcerum et oris fœditatem superaddis." To this, I shall subjoin what Leo, the African, says:—"This (the French disease) was not known in Africa before the time that King Ferdinand drove the Jews out of Spain; it is looked upon as an undoubted fact that it was brought from that country:" and he farther asserts, that it took its origin from the commerce which the natives of Africa had with the wives of the banished Jews.

Another historical fact throws some additional light upon this subject: it appears that when Grenada was taken by Ferdinand and Isabella that many of the Moors fled into Italy, and they are distinctly accused by Infessura as having imported the disease into that country. In the month of June, in the following year, that is 1493, the Spanish ambassador complained that the Pope (Alexander the Sixth) had received these fugitives into the city of Rome. In October it is announced that a Cardinal had died of this new plague; and early in 1494, the Pope wrote to Charles the Eighth of France, who was then preparing for his Italian expedition, that he had better delay his journey, as a great and new plague was then raging in Rome. But perhaps a still stronger proof of the position I maintain, may be gathered from the silence of Columbus himself, as well as that of his son Ferdinand, who wrote the history of his father's life, in which he gives a description of all the diseases which afflicted the Spanish adventurers up to

the year 1496, but there is no mention of such a disease as syphilis to be found in his work; neither do any writers on America, for the first 35 years, make any such assertion—the account given by Lopez de Gomara relating to a period long subsequent to this. I shall beg leave to add one more consideration: among the numerous names given to the disease, upon its first invasion, no one ever thought of calling it the American disease, a most singular omission, if the persuasion of its West Indian origin had been so universal as it was afterwards asserted to have been. I am unwilling to extend this part of my subject unnecessarily, and I shall therefore only observe, in reply to those who draw an argument for the contrary belief from the knowledge which the natives of Hispaniola had obtained of the virtues of the Guaiacum wood, and the cures they were enabled to perform by its means. Upon inquiry, we shall find that the first knowledge of this remedy in Europe was not obtained until the year 1508, rendering it very probable that the discovery of the virtues of the Guaiacum was but recently made by the Indians; for had they known it previously, there can be little doubt that the knowledge of the disease, (granting its American origin,) and its remedy, would have been communicated nearly at the same time. Upon this, Swediaur remarks very properly, that should the natives of Otaheite discover some remedy for the venereal disease, it would be quite as fair to conclude that, therefore, it must have been endemial in that island. It would be easy to produce many other passages from contemporary authors, tending to establish the position that I have assumed, namely, that syphilis was known in Italy several years prior to the return of Columbus from America; and that the invasion of Italy by the French, followed by the siege and capture of Naples, where the troops of several nations were assembled in great numbers, served only to render its progress more rapid, and to spread it in every direction throughout Europe; for we find, that in 1497, it had acquired so much importance, and become so serious an evil in Paris, as to give rise to an *arrêt* of the parliament of that city, by which, among other regulations, it is ordered that every person not actually residing in Paris, should, when seized with the disorder, after the date of the proclamation, go out of the city, to the country or place of their birth, under the penalty of death; and a few months later, that is in September, James the Fourth of Scotland found it necessary to issue a proclamation, banishing all persons afflicted with the *grand gore* to the island of Inch Keith, over against the town of Leith. I have now said enough, I trust, to show that there are sound reasons for doubting the commonly received opinion of the American origin of syphilis; but it may very reasonably be asked, from whence then did the disorder proceed, since it is quite evident that its origin must be dated at no very great distance of time from that period, and that it then first began to excite a great, but well-founded, alarm

among all the nations of Europe? This, then, will form, in part, the subject of my next Essay.

From the Glasgow Medical Journal.

ON IRRITATION OF THE SPINAL NERVES. By THOMAS BROWN, M. D. Senior Physician to the Royal Infirmary of Glasgow.*

In the following essay, I purpose to offer a few observations on the symptoms and treatment of some diseases of the spine, which are by no means of rare occurrence, but which appear occasionally to be overlooked or neglected.

I allude chiefly to those morbid affections of the spinal nerves so often met with in young females, and occasionally also, although much less frequently, in women of more advanced life, and in males.

In most instances, certainly, this irritated state of the nerves is not entitled to the name of serious disease, since the symptoms are not acute, and since they are easily removed or prevented; but when it is allowed to remain for any considerable length of time, it often produces nervous complaints and general bad health; and, of course, it becomes infinitely more unmanageable.

It occasionally happens, too, that this affection of the spinal nerves, even from its commencement, is much more serious in its nature, and instead of being local or strictly confined to one spot, extends to other parts of the spine, or even to distant organs, and assumes an appearance particularly distressing and obstinate. Still, however, as the symptoms which attended this more formidable disease arise from a local affection of the nerves, I have ventured to treat of it in this essay. This form is peculiar to females.

Although I have had numerous opportunities of attending to the symptoms and treatment of these affections, in the Lock Hospital of this city, as well as in the Royal Infirmary, and in private practice, yet I find that this essay must remain exceedingly imperfect. I trust, however, that the involved and vague nature of every subject connected with nervous diseases will serve as a sufficient excuse.

I find some difficulty in giving a name to this disease, but as it consists, perhaps, in a state of increased irritability in some of the spinal nerves, we may name it SPINAL IRRITATION.

It is proper to remark, in a very general way, that the complaints at present under consideration differ materially from some diseases which are exceedingly apt to be confounded with them, and which occasionally affect the spine along with spinal irritation. I allude to lateral curvature, and to inflammation of a portion of the spine, either from accident or

* Five years since, viz. in January 1823, the following Essay, nearly, was read to the Medical Society of this city.

from other causes. These diseases of the vertebræ are by no means uncommon at the age which is most liable to this other complaint, and instances do occur in which the two diseases are combined. In C. D., in particular there was ultimately decided lateral curvature of the spine, as well as spinal irritation; and I have seen the same combination in other patients. I conceive that this fact may explain some of the anomalies of this perplexing disease.

After I have given some examples of spinal irritation, I shall mention two or three other cases, in which, as well as in that disease, the chief pain was felt at the sentient extremities of the spinal nerves, but in which the local disease in the spine appeared to be different from that which existed in the other complaints.

In the first place, I shall give an account of one case out of a great number, in which spinal irritation occupied its most usual situation, and was marked by its most common symptoms. I shall then notice a few instances in which it was more complicated and varied in its form, though it occupied nearly the same part of the spine. I shall afterwards detail some cases in which the morbid affection occupied some other than the common situation, and was attended by unusual symptoms; and I shall conclude with giving some general remarks on these diseases, and on their treatment.

Miss C. aged 17 (September 1822,) of a robust make, and apparently in good health, for more than a year has complained of pain, situated below the left mamma. This has been fixed to one spot for nearly the whole time. It is a gnawing bruised feeling, increased materially by fatigue of any kind; and, after fatigue, it is attended with restlessness. It is relieved by reclining in the horizontal posture. It is not sore to the touch. The complaint has been treated by a surgeon in the country as a case of rheumatism. She has been bled and blistered for it, but without any good effect; and at last it has occasioned so much anxiety in the minds of her relations, that she is brought to Glasgow, from a considerable distance, for the benefit of farther medical advice.

On examining the spine, it is found to be perfectly of the natural shape and appearance; but when pressure is made on it, about the 7th or 8th dorsal vertebra, she complains of a considerable feeling of tenderness, amounting even to pain: and she finds that the uneasy sensation shoots forward exactly to the affected part of the breast. She had not paid any attention to this tender part of the spine; indeed, she had no idea that there was any thing faulty there, till her attention was called to it by the examination.

After I saw her, she had a dose of physic; ten leeches were applied to the pained part of the spine, followed afterwards by a small blister; and the horizontal posture was enjoined. She was nearly free of pain in a few days, and returned home, with directions to repeat the blister, and to avoid fatigue.

It would be needless to bring forward any

other example of the most common form and site of this disease, than the one I have mentioned. From the journals of the Lock Hospital, and from my notes of private practice, I could, I am confident, quote fifty cases nearly similar. Such instances occur frequently in the experience of every medical practitioner. There are, however, several varieties in those symptoms which attend the disease, when seated, as in the former case, in the lower part of the dorsal vertebræ. I shall mention those merely that I have seen.

The site of the pain in the *breast* varies much. It is needless to mention, that it is occasionally in one side of the chest, occasionally in the other; but I am confident that it is much more frequently in the right than in the left. It is sometimes within a few inches of the spine, but much more frequently it is nearer to the sternum, and occasionally it is immediately under this bone. It is generally described to be a weary or bruised pain. It is seldom increased by the touch, but sometimes, though rarely, it is tender when pressed. It is usually, though not always, relieved or even removed by the horizontal posture.

The tender part of the *spine*, on the other hand, in a great number of instances, is not attended to. It is not thought of till the affected part of the back is pressed, or till a sponge dipped in hot water be applied to it. In either case, a very sensible pain is felt, which, especially when the sponge is used, is occasionally acute and continues for some time. In general, pressing the spine not only occasions pain in that part, but the pain penetrates to the affected spot of the chest, thus distinctly proving the connexion between the two. It often happens that pressure on the spine occasions a feeling of oppression more than of pain in the chest.

The pained part of the spine, in general, does not exceed an inch in diameter, though it occasionally happens that the uneasiness extends either above or below the tender part, appearing to radiate from it as from a centre, sometimes to a considerable distance. This pain of the side, excited by pressure, is almost in every instance felt on the same side of the spine as the pain in the chest. That is, if the patient have pain in the left side of the chest, then the left side of the spinous process of the vertebra is more tender than the right, and *vice versa*.

There is seldom any fever with these symptoms, although I have repeatedly seen instances in which there was a considerable degree of febrile irritation, requiring bleeding and other evacuations.

Cough is not often present, although there is occasionally violent convulsive cough, very noisy, and not attended with any expectoration.

In a girl, a patient in the Lock Hospital, there was pain on the left side of the sternum, as if the part had been bruised. This was distinctly connected with a tender state of the spine below the middle of the dorsal vertebræ. Along with these common symptoms, there was severe pain and numbness extending down

the left side of the abdomen near the spine to the groin, and down the thigh, following nearly the direction of the anterior crural nerve. There was also a considerable degree of febrile irritation.* In two other cases, along with the tenderness of the spine, there was an extended and superficial feeling of rawness and pain over one side of the thorax, which even extended to the abdomen. It was described as being a feeling as if the skin were abraded. This pain was permanent. Blisters, fomentations, and frictions, had been tried in vain. It yielded, though not till after months, along with the complaint in the spine, to the horizontal posture, and to an issue in the back.

In each of the instances already mentioned, this affection of the spinal nerves was situated nearly about the 8th or 9th dorsal vertebræ, in which situation, as far as my experience proves, the disease is more frequently met with than in any other. It is obvious, however, that the symptoms attending a complaint of this description will be quite influenced by the portion of the spine which is affected, since the nerves have different distributions and functions, according to their origin.

Next in frequency to the middle of the back, I have observed this disease in the upper part of the neck, about the 2d or 3d cervical vertebra.

In one very complicated case, there appeared to be two sets of symptoms, each of which radiated from a tender part of the spine, as if from a centre. In this young lady, Miss M., there was pain in the left side, and numbness and pain of the left arm and leg, distinctly connected with tenderness situated about the lower of the dorsal vertebræ. This partial palsy, however, was strictly confined to one side. There was neither the pain along the insertion of the diaphragm, nor the paralysis of both of the lower extremities, which frequently attend caries or protrusion of the spine. There was also, after some time, a similar state of the 2d or 3d cervical vertebra, which occasioned pain in the left side of the neck, commencing between the angle of the jaw and the mastoid process. The pain, with great acuteness, also extended upwards from the neck to the back part of the head, and even to the forehead, following the ramification of the 2d or 3d cervical nerve; and on several occasions, probably by some nervous communication or sympathy, it induced an alarming degree of torpor or insensibility, which continued even for days. These two sets of symptoms, connected with two different portions of the spine, were so much mixed together that it was difficult to determine which belonged to one irritated part, and which to another.

* A. S., a girl of 17 years of age, is at present (December 1827) a patient in the Glasgow Royal Infirmary. Her case is a well marked one of spinal irritation, chiefly in the middle of the back; but besides this, there is numbness and weakness of one arm, nearly amounting to palsy.

Posture alone was tried, but it was not found to be sufficient. As the paralysis of the arm and leg were the most urgent symptoms, issues were opened in the back, on each side of the pained part. The pain of the back and of the chest, with the palsy of the arm and leg, soon yielded. The headach, pain of the neck and shoulders, and fits of stupor, also disappeared, and it was presumed that the dorsal irritation had been the source of the whole disease. After reclining in the horizontal posture for three months, she was allowed to get up from her confinement. On the first, or at most, on the second day, however, after sitting up for a short period, it appeared that the cervical nerves were still in fault, the morbid symptoms arising from which had been merely suspended by the horizontal posture, but were again called into activity by the change to the erect position. The left side of the cervical vertebræ, near the head, again became acutely tender to the touch. The mastoid muscle on each side was rigid and projecting, so as forcibly to attract attention. The pain extended downwards to the left shoulder, and even along the back, nearly to where the issue had been, so as to create the decided impression, in the minds of the lady and of her friends, that the whole disease had returned. The pain also radiated upwards, over the integuments of the head, even to the forehead. The neck was altogether so tender, that it was not easy to say exactly where the morbid affection was situated, for neither in this, nor in any of the former cases related, was there the slightest inequality in the form of the spine, as far as could be discovered by the touch. As this patient had a decided objection to the use of blisters, from repeated experience of their inefficacy in removing her complaints, and from feverish feelings which they excited, and as she had been materially relieved by the issue in the back, a kidney-bean was inserted into an incision on each side of the spine, near the head. These issues occasioned great irritation. The original pain, shooting up and down, continued, and to this were added nervous feelings, and at last she got into a state of insensibility, which continued for two days. It was necessary to remove the issues. Soon after, I ventured to apply a blister to the nape of the neck, but, contrary to former experience, it acted like a charm, and relieved her from a state of suffering which had continued for a month. It was now discovered that the dorsal vertebræ were quite free from pain, and that the pain which had extended to the back had merely radiated from the neck. After the blister healed, the affected portion of the spine seemed to be limited to a small spot, on the left side of the 2d or 3d cervical vertebra. The pain again returned severely, shooting up over the head. A blister now had no effect in removing it. It was so acute, that it was judged necessary to pass a cord through the part. In a few days, erysipelas commenced around the seton, but almost immediately the severe pain of the neighbouring parts yielded. The erysipelas

extended over the head and neck, with smart fever. It then attacked the lips, and entered the mouth. The tongue became inflamed, swelled, and hard, so as to be nearly immovable. Immediately after, cough came on, as if the inflammation had extended down the windpipe to the lungs. This ended in phthisis pulmonalis, of which she died, after suffering in the usual way for several months. It was remarkable, that whenever the erysipelas commenced, the spinal irritation subsided, and she continued perfectly free of pain of back, or of any of her former ailments, till her death.

I have given an account merely of those symptoms which had been present in this patient for 18 months preceding her decease. She had suffered for a great length of time before that period under various anomalous symptoms, many of which, I am confident, had been connected with irritation of one portion or other of the spine. At one time, caustic issues in the loins had been of great use in removing a number of uneasy feelings, which appeared to radiate from that part of the spine; but I am confident, that for a much longer period the neck had been in fault, and that various nervous feelings, with repeated fits of insensibility, followed by total blindness, which lasted for several days, were connected with that irritation, although she had so many other complaints, that it was difficult to say what was the leading disease. I am certain, however, that confinement to the horizontal posture had the effect of giving great relief, and of simplifying the disease. It rendered the symptoms so much milder, that a number of nervous sympathies were prevented, and in this way allowed the attention to be fixed on those points of the spine, from which, in my opinion, all her other complaints, except the last fatal one, emanated. It should have been remarked, that in this case the digestive organs were healthy. Her appetite and general appearance were good. Her catamenia were quite regular, till phthisis pulmonalis came on. It should be noticed, too, that there was not the slightest curvature or projection of the spine.

When the affection of the spinal nerves is situated about the middle of the lumbar vertebræ, it is apt to occasion severe pain in some part of the abdomen. I have seen it of a spasmodic nature, attended with flatulency, and occupying apparently the arch of the colon; and in one case, it seemed to be fixed in the caput coli. In these cases, the severity of the complaint may appear to call for powerful evacuations, from the apprehension that inflammation exists; whereas, if the attention be directed to the share which nervous irritation from the spine has in increasing the acuteness of the pain, very active depletion will not appear to be essential.

M. M. was a patient in the Lock Hospital, in May 1821. During her confinement under a mercurial course, she complained of acute pain nearly in the situation of the caput coli, with costiveness, and occasional vomiting, a

quick pulse, and other feverish feelings. She was repeatedly bled, blistered, and purged, and took magnesia in large doses, with great benefit. This complaint, however, returned severely every two or three days; but at last it was ascertained that there was a marked pain about the middle of the lumbar vertebræ, and that whenever this part was pressed, the uneasiness extended forwards, exactly to the affected part of the abdomen. Attention was now paid to the spine. The part was cupped and blistered, with immediate relief to the pain. The horizontal posture was rigidly enforced. She had afterwards a return of this complaint. The same means of cure were employed, and she got well, much more rapidly than when powerful evacuations were used. I should have mentioned, that the tenderness of the abdomen to the touch, appeared to confirm the idea that the part was in a state of inflammation.

In the following very complicated case of spinal irritation, there was not the least appearance of protrusion or of irregularity in the shape of any of the vertebræ; but there were three, or even four separate parts of the spine in fault. Sometimes one of these parts was in an irritated state, sometimes another, sometimes two of these were affected at one time; and according to the situation of the irritation, the symptoms varied.

A. R., aged 23, was admitted into the Lock Hospital, in May 1821. She had primary venereal complaints of little consequence; but besides these, she had completely lost her clear voice for several months. She could only speak in whispers. After being in the hospital for some time, this symptom was attended with pain at the larynx, and with dyspnoea. It was ascertained that one of the upper cervical vertebræ was tender to the touch, and when this was pressed, the pain extended forwards, so as to increase the pain in the larynx. Leeches and a blister were applied to the neck, and in two days the pain of the larynx and the dyspnoea were removed, and she at the same time recovered her clear voice, which had been lost for three months.

It would be needless to enumerate all the minutiae which occurred for eight months in a most complicated case; but I shall mention merely the general results.—About the same time with the former complaints, the lumbar part of the spine was affected in two separate points. There was pain within the abdomen, extending upwards and downwards, which became severe whenever the spine was firmly pressed. There was also pain in voiding urine, pain in both groins, palsy of one lower extremity, and at last complete retention of urine, so as to render the use of the catheter necessary for three weeks. These last symptoms seemed to be connected with a different part of the spine from that formerly mentioned, since pressure on this other tender portion of the spine excited the pain in the groin and in the urethra. The retention of urine returned occasionally during the eight months she was a patient in the Lock Hospital.

At other times, one of the dorsal vertebrae was affected, with the usual well-marked pain in the thorax, and with violent pain in the stomach and abdomen. At last, the neck became again irritated, and soon after that the head. She was quite deranged, requiring the strait-jacket. She then became comatose for some days. She recovered from this state, by bleeding and blistering. For a time she remained quite blind of both eyes, after that of one eye; but ultimately she recovered the use of both. The catamenia now returned profusely, after having been suppressed for a year. There was also the discharge of much coagulated blood from the uterus. After this she gradually recovered strength, and was dismissed on the 3d of December almost well.

These various affections, all immediately arising either from disease in the spine or in the head, returned more than once, and they gave rise to a strange combination of nervous and other complaints. Copious bleeding, blistering, caustic issues in two separate parts of the spine, purging, and other means, were necessary at the time, and occasionally they were of service. These complaints ultimately subsided, after the return of the menstrual flow, which had been suppressed for a year.

Nearly allied to the case of A. R. is that of C. D., though it differs in some respects. In this girl, there was decided lateral curvature of the spine, though not to any remarkable degree. She was admitted into the Lock Hospital, in July 1822, with primary venereal sores. She had a singular pink complexion, which augured some peculiarity of constitution. She was easily flushed and made feverish, but was in a tolerably good state of general health. Her catamenia were quite regular, and in this respect she differed materially from A. R.*

She soon began to complain of pain in two separate parts of the spine. *First*, in the 9th or 10th dorsal vertebra. When this was pressed, the pain extended up the spine, even as far as the head. She had also occasionally a burning shooting pain in the head. *Second*, in the 2d or 3d lumbar vertebra. This was really a very severe complaint. The pain extended forwards to the edge of the ribs on the left side, and down the outside of the thigh. There was also a painful feeling of numbness of the whole of the left lower extremity, producing complete lameness. These symptoms, in some respects, resembled nephritic pains, but there was no stomach complaint, nor apparent disorder of the urinary organs. She was fixed to one posture in bed by this

* I may here mention, though it is not much connected with this subject, unless to prove the peculiarity of her constitution, that although her bed was placed in an airy and large ward, and close to a window exposed to the west, she soon took hospital gangrene. It attacked a boil on the arm. This made rapid progress, and was attended with all its usual characters. This sore had never before appeared in the Lock Hospital.

last complaint. The slightest attempt to move was agonizing. By various means, these complaints were materially abated; but on the 24th of November, when confined to bed in the horizontal posture, a new part of the spine became irritated. She complained of pain in the right side of the third cervical vertebra, extending down the neck, which was actually tender to the touch, to the top of the right shoulder, and along the arm to the wrist. This pain also extended upwards to the crown of the head, on the same side. It did not affect the left side, either of the neck or head. She described this pain as more severe than any she had ever before felt; and even worse than the burning pain of hospital gangrene. The pain was always present, but the severity of it increased five or six times through the night, and during this exacerbation, which continued for about fifteen minutes, she became quite blind. The sight returned gradually, with the appearance of luminous stars floating before her. After this new pain attacked her, she vomited, with a bitter taste, every thing she swallowed, and even with such straining, as to be mixed with blood. By copious bleeding, the severity of these symptoms abated, though the vomiting occasionally returned. A seton was passed through the neck.

It is worth remarking, that by the report of the 14th December, it appears that she and her fellow-patients observed that she had an unusual degree of rigidity and projection of both of the mastoid muscles, and that this spasmodic action preceded the attack of vomiting, and the increase of pain down the shoulders.

On the 27th of December, the day after a number of leeches had been applied to the neck, on the idea that the vessels of the spine were in an enlarged state, she became much worse. The pain spread over all the head, with flushing of the face. The stounding pain in the head appeared to threaten an effusion on the brain. Erysipelas came on over the face. The cord began to discharge, and she became very materially relieved.

This, then, is another instance of material relief, arising, in a severe complaint of this description, from an attack of erysipelas. It may be remarked, that at different times, whenever the irritation in the neck was increased, she rejected from her stomach any thing she had swallowed. This, however, in general, was merely eructation without nausea; though occasionally there was nausea and vomiting, with severe straining.

This patient at last recovered so much, that she left the Hospital in a state of moderate ease. Her back was weak, and there was distinct lateral curvature of the spine, though not to any great degree.

Remarks.—I am fully sensible of the difficulties which surround every inquiry where the nervous system is concerned, and of the little chance that exists of our ever being able to form views on this subject, materially more correct than those which we possess at present.

But although the precise nature of the connexion that exists between these nervous fibres and the other parts of the animal system, be quite unknown, yet we are certain that this union is absolutely necessary for sensation, for motion, for the performance of every function, and, of course, for the existence of life. No one doubts that all those necessary actions, which are constantly going on in the viscera of the thorax and of the abdomen, as well as in every other part of the body, are immediately and necessarily under the influence of the nerves.

To guard against those dangers, which would arise from the interruption of this necessary influence of the nerves, there are no doubt numerous communications and networks; but still we are certain, that each individual nerve is of some use, either of greater or less importance.

We can therefore easily conceive that pressure, or any other irritation, applied to the spinal nerves, should produce very sensible effects, either in occasioning pain, or in deranging the healthy action of the muscles, or of the viscera, to which they are distributed. We find accordingly, that in the cases related, such derangements did result from those causes.

The highly important results which have followed from the brilliant discoveries of Mr. Charles Bell, on the functions of nerves, have already thrown much light on many points connected with the nervous system, which before his time were involved in obscurity. It is exceedingly probable that these cases afford examples of diseased states of the two sets of spinal nerves, and that future experience may enable us to arrange these into separate classes. I merely wished, on the present occasion, to place in contact with each other, the different varieties of these diseases, chiefly in females, which have occurred in the course of my own practice, within a short period, as a sort of nucleus around which, in future, I might dispose other examples.

In several instances, in males, I have seen a disease in some respects similar to the one which is here named spinal irritation, arising, as in that complaint, from irritation applied to the origin of a single nerve at the spine, and corresponding also with it in several other particulars. This, however, in males, is a rare disease, and when it does occur, we can generally, though not always (since it occasionally arises from acidity of the stomach or of the duodenum) trace it to curvature of the spine, or to inflammation resulting from some injury which the spine has sustained.

These affections of the spinal nerves appear to divide themselves into two classes:—

1. Those in which there is merely a morbid sensibility of a single nerve.
2. Those in which there is a more general and constitutional irritability, in which the irritation is apt to affect different parts of the spine in succession, and to occasion a whole train of singular symptoms.

This last form, I believe, is confined strictly to females, the other is occasionally met with in males. Those two forms of spinal disease, though generally separate, are occasionally found to be so much combined, that it is difficult to say what symptoms belong to one form, and what to the other.

A great proportion of the examples of the first class which I have seen, have been in female patients in the Lock Hospital. Perhaps nine-tenths of the inmates are young women, between 15 and 20 years of age. At this important period of life, when the constitution has only lately changed, and when the growth of the body is perhaps more rapid than at almost any other, it is obvious that intemperance in all its forms, and want of proper air and exercise, will have most material influence in deranging the health. A great number of these females, many of them slender and delicate, and without proper clothing, are exposed to the inclemency of the weather at all seasons and hours, perhaps in a state of intoxication; or not unlikely, they remain at home ruining their health by hard living, and suffering as much by confinement to a close room, as they would by exposure to cold. Under the operation of these various noxious causes, we are not to wonder if the spine should frequently become affected, and we accordingly find that a number of those females who are admitted for the cure of venereal diseases, suffer from affections of this nature. This complaint is rarely met with in country girls, whose muscles and solids are invigorated by their mode of life. We can easily conceive, indeed, that the muscles arranged along the spine should be most materially strengthened by bodily exercise, since on every exertion they must be constantly and powerfully in action, in altering or even in preserving the erect position of the body.

I have to repeat what was formerly briefly observed, that I consider these cases as very materially different from those which are known to arise from inflammation or caries of the vertebræ. These latter diseases are met with more usually at a much earlier period of life than the former. They occur perhaps with equal frequency in either sex. With them, there is often paraplegia, or perhaps a rigid contraction or spasm of the muscles of the lower extremities. The urinary bladder, or bowels, are frequently torpid or paralytic. The spine protrudes or is irregular in its shape, and there is a feeling of debility, or of pain, extending along the insertion of the diaphragm to the ribs, which is very characteristic of a diseased vertebra in the most usual situation. In the disease, however, which forms the subject of this essay, and which we have named Irritation of the Spinal Nerves, there is no alteration in the shape of the spine, as far as can be observed by the eye, or by the touch. There is rarely any paralytic tendency in the limbs, bladder, or bowels; and if there should be any weakness of the extremities, which is rare, this is confined to one side of the body. It is besides, in general, a disease of greatly less danger than the former, though an ex-

treme case of the second class is certainly a very tedious and formidable complaint.

In this disease of the spinal nerves, I have rarely seen the pain of the chest in both sides of the thorax at one time; and it differs quite from that feeling of pain or of fatigue, which extends across the whole thorax, where the diaphragm is attached, and which is so frequently present when the vertebræ are inflamed or carious.

Since these two states of the spine differ so much from each other, we have next to inquire what is the nature of this difference.

I conceive that in the carious spine, the morbid symptoms arise from compression or inflammation of the whole spinal cord; whereas, in this other disease, the spinal marrow is uninjured, but we have some degree of pressure applied to the spinal nerves, as they issue from the vertebræ. We accordingly find that in cases of protruded spine, we have all those symptoms which arise from injury of the whole mass of the spinal marrow; whereas in the other disease, in its most simple form, we have only those complaints which arise from the compression of a single nerve. We have merely pain at the sentient extremity of this nerve, somewhere in the thorax or in the abdomen; and pain in the back, occasioned by the irritation of those nervous twigs which have the same origin as the larger branch, but which pass backwards to be distributed on the muscles on the posterior part of the spine. In cases of longer continuance, or of greater irritability, from nervous communications with the great sympathetic nerve, or with other nerves in the thorax or in the abdomen, various distant sympathies and anomalous nervous feelings are brought into action; and, since the proper action of every organ depends on the influence of the nerves, ultimately the organs of digestion, and the various glands in the abdomen, are deranged in function, and at last in structure. We accordingly often find that such changes are met with in severe cases of this disease.

The frequency with which this painful affection occurs in delicate constitutions, where growth is rapid, can perhaps be explained if we keep in mind the complex and varied structure of the spine. This organ is composed of a series of joints, furnished with numerous ligaments and muscles; for we are to consider each bundle of muscular fibres as a muscle, with an independent action, although the anatomist brings whole classes of them under one name. There are many separate muscles, for instance, comprehended in the multifidus spinæ, and in the sacro-lumbalis. The motions of the spine, too, are frequent, and it has, independently of occasional and great exertions, the weight of the upper parts of the body to support. It is apt to suffer from sprains, and from wrong posture of any kind. Even when lying in bed, it is not exempted from the effects of over-exertion, or of wrong position. How often do we find these complaints brought on in delicate girls, or increased by the unequal surface of a feather bed, or of a

bolster which is too high and firm! Even in many adults, who are by no means feeble, we find that a soft feather bed occasions pain in the back and in the neck; but if a feeble growing girl be confined to bed by any cause, the spine will be much more liable to become sprained, or perhaps even misshapen, from any inequality in its surface. It is reasonable to suppose, that from any such cause, especially if frequently repeated, or long continued, one or more of the ligaments or muscles of the spine should be stretched or strained. A slight inclination to one side, probably only at one joint, will be the effect, and in this way the nerves issuing from one side of the spine, surrounded with a bundle of fat and cellular membrane, will be compressed and irritated.

That this slight inclination to one side is the cause of the pain, in the *simple form* of the disease, appears to be proved by the fact, that only one of the spinal nerves is affected at the commencement of this disease. There is pain merely in a small space, on one side of the thorax, and there is more uneasiness on that side of the spine which corresponds with this pain, than on the other. This distinctly proves the limited nature of the disease at first. Most likely, after some time, some degree of inflammatory action may result from the pressure on the nerve. Lymph will be effused, and thus the pressure on the nerve will be increased. Besides, the muscles and ligaments of the spine become more strained. The intervertebral cartilage on one side becomes diminished by pressure and by absorption; on the other side it is proportionably thickened. We may conceive, therefore, that after a certain time, even some degree of irregularity in the shape of the spine may be occasioned. The disease becomes more serious, and more difficult to remove; and a greater number of nervous sympathies are brought into action.

As a farther confirmation that this view of the nature and origin of this most common form of the disease is correct, it is observed, that at the commencement the pain of the breast goes off whenever the patient lies down in the horizontal posture, but that after the complaint has been fixed for some time a change of posture alone does not so certainly relieve the uneasiness in the breast.

We have found that the affections of the spinal nerves occurs much more frequently in the lower of the dorsal, or in the upper of the lumbar vertebræ, than in any other situation; and by looking to the form of the skeleton, we can readily account for the greater liability of this part of the spine to this disease. The ribs, united to the dorsal vertebræ and to the sternum, form the whole chest, as it were, into one piece. From this union of the different parts, we observe that there is but little motion in that portion of the spine which forms the posterior part of the chest. On the other hand, the lower lumbar vertebræ are larger than those above, and are fortified by strong muscles. Besides, the motions of the lower lumbar vertebræ are very limited, from

being fixed to the pelvis, which may be regarded as a firm support. There is, therefore, a greater extent of motion in that portion of the spine which is placed between these two fixed points, than in any other, and we accordingly find that affections of this nature are much more frequently met with about the lower dorsal, or upper lumbar vertebræ, than in any other part. This weak and moveable portion of the spine has also to support the whole weight of the upper part of the body. It may be considered as the centre of motion.

The nerves of the second or third cervical vertebræ also frequently suffer from diseases of this nature. This part of the spine is nearly similarly situated as the upper part of the lumbar vertebræ, since we are here to consider the chest as the fixed point, corresponding to the pelvis. To this the lower of the cervical vertebræ, surrounded by strong muscles, are fixed, while the upper part of the neck has a more free and unrestrained motion.

It is exceedingly probable, too, that the abrupt shape of our bolsters and pillows increases the disposition to disease at this part of the spine.

We shall now revise the various cases, and make some observations on the different symptoms which occurred in each.

When the disease is fixed in its most usual situation, namely, near the lower part of the dorsal vertebræ, the pain is felt at a distance from the spine, in some part of the thorax. Occasionally, however, there is merely a feeling of oppression in the chest. This distant pain is certainly connected with the distribution of the sentient extremities of the affected nerve; for, in other instances, irritation of the trunk of a nerve is felt where the nerve is ramified. In many other instances, then, in these affections of the dorsal nerves, we find that when the origin of a nerve is irritated, the pain is chiefly felt at the sentient extremity of it; and a knowledge of this fact is frequently of great use in rendering our practice efficient. We find, indeed, that local applications to the seat of the pain have nothing but temporary effect; whereas, when our attention is directed to the distant cause, we often succeed by the most simple means.

Some time since, I was requested by a medical friend in town to visit, with him, the captain of a coasting vessel, who ten days before had received a severe blow on one of his thighs. The pain and shock were considerable at the time; but he was out of bed, and apparently recovering, till four days before I saw him. Then he complained of a burning feeling in the groin of the bruised thigh. This had become very acute, and was attended with spasms, apparently of the sartorius muscle, excited by the most trifling motion. He was literally fixed to one posture in bed, from the agonizing pain and spasm, produced by any attempt to move. Leeches in great numbers had been applied to the groin. He had been freely bled, and had used opium, laxatives, &c. without any relief. On examining the spine near the sacrum, a considerable degree of ten-

derness was detected there, and when this was touched, the pain extended down to the affected groin. Leeches and a blister to the sacrum gave immediate and complete relief. In fact, one of the lumbar nerves, distributed to the sartorius and neighbouring muscles, was irritated and inflamed, at its origin from the spine. The pain was felt at the sentient extremity of the nerve; but local means, applied to the spine, where alone efficient in removing the complaint.

When there is merely the feeling of oppression in the chest, this may perhaps be connected with pressure applied to the nerve of motion, instead of the nerve of feeling; since, from the experiments of Mr. C. Bell, we know that the two nerves which arise from each part of the spine have different functions. The relief of this pain by the horizontal posture is readily explained, by the removal of pressure from the trunk of the nerve, as it passes between the vertebræ.

It has been remarked, that the pain which is perceived when the back is pressed, is connected with that branch of the spinal nerve which passes backwards to be distributed to the muscles of the spine. I have seen several instances in which all the symptoms of this disease were present, with the exception only, that there was no pain when the spine was pressed. There was local pain in the side, increased by fatigue, and relieved by posture, and applications to the spine. In these cases, I was inclined to theorize, in supposing that the nerve of feeling which passes forwards was irritated; but that the branch which passes backwards to the muscles of the spine, was free of irritation. The complaint which forms the subject of this essay is often so strictly local, that this supposition does not appear to me to be unreasonable.

Occasionally disorder of the stomach and bowels is joined to this affection of the spinal nerves, and appears to arise from it, since it yields, if the irritation of the spine is removed, and returns whenever this irritation is renewed. On the other hand, however, I have seen the spinal irritation, with pain of the breast, yield at once to the removal of a load from the bowels by an active purgative, showing distinctly the immediate connexion that exists between the spinal nerves and the intestines; and proving that, on some occasions, the affection of the spine disorders the digestion, but that, in other instances, indigestion is the primary disease, and the spine merely sympathizes with it. In several instances, there was an uncommon degree of acidity of stomach joined to this disease. This had been treated without effect in the way that was effectual after attention was paid to the spine, as well as to the stomach, so that I was disposed to conclude that the excessive acidity was connected with the spinal irritation.

We occasionally meet with examples of dry, loud, convulsive cough in females whose spinal nerves are irritated. I believe, however, that this cough is not occasioned directly by the irritation of the spine, but that the stomach and

bowels are deranged by this irritation, and that the cough is the effect of this derangement. I form this conclusion from a number of instances of nervous or convulsive cough, which have occurred to me, in most of which, though not in all, a fault in the digestive organs appeared to be the immediate cause of this symptom.

In irritable constitutions, this local complaint is often the cause of a whole train of nervous feelings and sympathies, especially if, from inattention to the source of the disease, an attempt is made to invigorate the body by exercise. It is plain, that in such cases, we can scarcely expect to remove these symptoms, unless we attend to their origin, and that exercise, instead of removing the complaint, will increase and confirm it.

We find that many painful affections can be referred to pressure or irritation, applied to the trunk of a nerve at a distance from the pain. Some of these pains are confounded with rheumatism, or they get the vague name of nervous pains. It is of great consequence, to be aware how frequently these complaints depend on a distant cause. When numbness, and want of power of moving the arm or leg, are joined to this disease, we conclude that these symptoms arise from the local irritation of the nerve, spreading to communicating nerves; not from any general affection of the head or of the spinal marrow. At least, in several instances that I have seen, I have not been able to discover any other cause. The palsy yielded to the treatment of the local disease in the spine. Paralytic complaints of this description afford a much more favourable prognosis, and are surely very different from that paralytic affection, which arises from injury of the whole of the spinal marrow, or from that which depends on injury of the brain.*

* Since writing the above, I have had one well marked case of spinal irritation in the Infirmary, (A. S., a girl of 17 years of age,) in whom there was, in addition to the usual symptoms of this disease, palsy nearly complete of the left upper extremity. The arm was numb, and she could only move one of the fingers. She also lost the power of swallowing any thing but liquids. These two symptoms remained, though almost every other yielded to the means employed. She had no pain in the head at this time, at least no fixed pain, nor any apparent affection of the mind. At one time, the sight was dim and the pupils dilated; but I conceived that these, as in other cases, were owing to spinal irritation of the nerves of the neck, not to any affection of the brain. The obstinacy of these symptoms, however, (for the arm had been paralytic for six weeks,) induced me to apply a large blister to the head, and certainly immediately afterwards the local palsy, the dysphagia, and the other symptoms of spinal irritation, which were lurking about her, yielded completely, and she very soon left the Hospital nearly recovered in strength, after having been three months a

We occasionally observe singular pains and feelings, and spasmodic twitches, about the neck and shoulders, arising, no doubt, from the nervous communication of these parts. I recollect to have seen two cases in which the cervical nerves were affected, in which the prominent feeling was that of a hair, stretched across the throat, occasioning much irritation and frequent inclination to retch.

Some years ago I attended a medical gentleman, whose symptoms depended on local irritation of the nerves of the neck, by a cause different from spinal irritation, but corresponding with it in producing action at a distance. In this gentleman, there was a most violent convulsive action of the right trapezius muscle, occasioned by a small tumour, irritating the accessory nerve of Willis, as it passes through the sterno-mastoid muscle. This spasmodic action or convulsion began in the right trapezius muscle, but it set other muscles into action, and the effect was most singular and distressing. He shrugged up his shoulders with such force and frequency, that in a short time he became quite exhausted by the painful exertion. This disease continued for many years, occurring at uncertain times, both day and night, rendering confinement to the house, large doses of opium, and retirement from business, absolutely necessary. At last it

patient in the house. From the result in this instance, I shall certainly in future, in these anomalous cases, direct my attention more particularly to the head, and, if obstinate, have no hesitation in bleeding and blistering this part. The head and the spine are so immediately connected, that affections of the head occasionally imitate those of the spine. Dr. Baillie, for instance, has ascertained, that paraplegia, in many examples, may depend on an affection of the head; and on the other hand, we occasionally find that a disease of the spine produces derangement of the functions of the brain. From this analogy, and from what occurred in the case of A. S., I would have no hesitation in these cases, in directing my attention more particularly than I have formerly done to the head. We know that fluids on the surface of the brain readily pass into the theca vertebralis. In a case at present in the Infirmary (H. K.,) the head was affected to an alarming degree with stupor, tinnitus aurium, and pain, for ten months. The primary disease, however, appeared to have been seated in the lumbar spine, and accordingly all these affections of the head gradually yielded to an issue on the diseased vertebra. I therefore again repeat, that in these cases of spinal irritation, we ought to be aware that the whole train of symptoms may depend on disease of the head, as well as of the spine. The affection of the sight, so common in these complaints, will be well explained by this view of the subject, and perhaps even the partial spasm of the muscles fixed to the spine, which we shall afterwards point out as being much connected with these ailments, will receive illustration by the same idea.

gradually subsided; and I believe that this gentleman has been free of the complaint for several years.

There is no part more frequently pained, from nervous irritation, than the pericranium or scalp. The pain is often acute, and if we examine the parts minutely, we find that it shoots along the various nerves which are ramified over this tendinous membrane. If we press immediately behind the mastoid process, the pain passes upwards along the cervical nerve which takes that direction, and ramifies even as far forwards as the forehead. The frontal branch of the fifth pair of nerves is often similarly affected, from cold or from disorder in the stomach; and the temporal branch of the fifth pair, when the salivary glands are irritated by mercury. I allude chiefly, however, to the first mentioned of these painful affections of the scalp, viz. that which shoots upwards from the neck, and passes behind the mastoid process over the head. This is a frequent complaint in delicate girls, and is occasionally acute. It often takes its origin from irritation at the origin of the nerves between the vertebræ, in consequence of the sudden bend of the neck from lying on a high bolster, and is remedied by very simple means. It is occasionally rheumatic, and is owing to exposure to cold. Sometimes this pain of the neck and scalp is peculiarly painful and obstinate, as in some of the cases mentioned.

In a great number of these cases, in which the cervical nerves are in a state of irritation, the sight is for the time injured. When the pain shoots from the neck over the head, there is generally either great sensibility, or in other instances great insensibility to light, with dilatation of one or of both pupils. The excessive sensibility of the eye is much more frequent than the opposite state, and it often becomes necessary in these cases to exclude the light entirely. I know of one young lady, who for years has been obliged nearly completely to darken the room; and I recollect of another similar instance, in which there is now a complete recovery. Indeed a great proportion of the cases suffer pain from the exposure of the eye to light. This sensibility and insensibility to light are apt to alternate in the same individual. In the lady mentioned above, as being much disturbed by excess of sensibility, the opposite state occasionally prevails; when every object looks dim and diminished in size, as if it were placed at a great distance. Some of these patients occasionally have the sensation of a green or of a blue light before their eyes; and they remark, that the intolerance of light is more remarkable, when the irritation in the neck is most severe.

It was stated, that in one case the pain was followed by fits of insensibility, which even remained for days.

It was also mentioned, that in another case, a severe attack of pain of the scalp was followed by complete blindness. This state continued for some time; after which it subsided.

These fits of pain and blindness occurred even so frequently as five times in the course of the night. In a young medical gentleman, who consulted me some time since, the pain of the scalp had nearly the same direction as in the former cases. It came on in fits, and during these he became blind. After some time the severity of the pain went off, and along with it the affection of the sight.

It was formerly remarked, too, that Miss M. and A. R. were completely blind for some time. It appears, therefore, that this symptom is not unusual in cases of spinal irritation. I cannot, however, explain, by any nervous communication, how this should happen. I can only say, that I have observed partial or total blindness in several cases, in which the upper cervical nerves were in an irritated state; and from the frequency of its occurrence, I must suppose that it was connected with this irritation, and arose from it.

As this symptom occurred, in general, during my absence, I am not sure whether, in all the cases, the pupils were dilated or not, during the state of blindness. In A. R.'s case, and in that of A. S. they were dilated, and I presume that such was also the case in the others, as that was the character of the eye when I saw it next day.

In at least two of these severe cases, there was almost immediate vomiting of every thing that was swallowed. Perhaps eructation is a better term than vomiting, since there was nausea only occasionally. This symptom was most likely connected with irritation of the 8th pair of nerves, or with that of the phrenic. The food was eructated quite unchanged in its appearance. From having seen this symptom present during the acute stage of these diseases, and subside whenever the irritation abated, I should feel inclined to examine the spine carefully in other instances, where eructation of the food or vomiting was a prominent symptom. In the case of C. D., every sort of food for days together was almost immediately rejected from the stomach.*

In taking a review of the different cases, which have been brought forward, there appear to be two marked varieties. We have, in the first place, a simple form of the disease, of very frequent occurrence, and easily removed, in which the morbid affection seems to be limited to a minute space. There is pressure merely of those nerves which pass

* I have lately seen two cases of spinal irritation, in which vomiting was a very prominent and urgent symptom. In the case of E. M., who is still (1828) a patient in the Infirmary, there was constant and even alarming vomiting of every thing swallowed, joined to irritation of various parts of the spine, and ultimately of the neck. This last was relieved by a blister, but the vomiting was not. It resisted anodynes, blistering the stomach, &c., for weeks, but at last it yielded at once to an anodyne enema, which was occasionally repeated to prevent the return of the vomiting.

out between two of the vertebræ. But there was another form of spinal irritation, which varied considerably from the other, and was infinitely more serious and obstinate. I allude to that more extensive tendency to disease, which has been observed in several of the cases.

Even in these, however, although the irritation wandered from one part of the spine to another, still the morbid symptoms at the time seemed to be confined to individual nerves, as they issued from the spine, not to be extended to the entire body of the spinal marrow. In several instances, these two complaints were so much combined, that we were unable to separate them, and we had only to give the history of the case, with all its anomalous symptoms.

We may entertain different notions regarding the nature of this singular form of spinal irritation, and it need scarcely be remarked, that it is of great consequence that the theory we form should be correct, since our practice may be materially influenced by it.

1. We might suppose that it is connected with a plethoric, or varicose state of the blood vessels of the spinal marrow. It is accordingly more frequently observed at the monthly period in women, than at any other time, and at this period we know that the blood-vessels of the lumbar region, at least, are in a distended and active state. We know, too, that this disease is remarkably moderated by local bleeding. The local and limited nature of the irritation, however, induces us to conclude that this theory is not the correct one. We find that it is confined chiefly to two parts of the spine, viz. to the middle part of the back, and to the upper part of the neck; and in these situations, frequently even to a single nerve with its ramifications. If, however, the disease depended on a plethoric state of the whole spinal marrow, or of a part of it, we ought to have an affection, much more general and extensive.

2. We might say that this disease was connected with relaxation of the muscles and ligaments of the whole spine, but that this weak state of the solids would be chiefly perceived in those parts where the motion is the greatest. This notion, however, will not account for the phenomena of these diseases. We often find great emaciation and debility of all the solids, without there being any tendency of this nature. In short, the disease is not confined to states of debility and emaciation. A. W. suffered severely from this disease, at last she was attacked with typhus fever, and was much reduced in strength by it. The spinal irritation went off whenever the fever came on, and did not return.

It is exceedingly probable that there may be some slight inequality in the position of one vertebra on another, as appears to take place in the more common and mild form of irritation of the spinal nerves; but still in this more serious form, there seems to be something added to this, which leads us to the supposition;

3. That the disease depends chiefly on pecu-

liar sensibility and irritability of the spinal nerves. We can easily conceive that this should be chiefly, or perhaps solely felt, at those parts of the spine, where the motion of one vertebra on another is the greatest, viz. at the loins and in the back. In the sound and healthy state of the nerves, the slight pressure occasioned by the motion of one of the vertebræ on another, is not sufficient to occasion pain; but when the nerve is too sensible, then this same pressure is sufficient to produce pain and irritation, varying according to the excess of sensibility. In very irritable constitutions, even the slightest motion may excite violent effects. In the cases described, indeed, the pain was occasionally very acute, even independently of motion; and was most likely connected with the spasmodic action of the muscles of the spine, drawing it to one side, and in this way increasing the pressure on the irritable nerve. In confirmation of this notion it may be stated, that in several of the cases, I have observed a marked stiffness and spasmodic state of the muscles of the neck. In two cases, it was remarked that the mastoid muscles, at one time, were rigid and projecting like boards.*

The more simple form of spinal irritation is not entirely confined to females; but it is much more frequently met with in them than in males. It is connected with the more delicate frame of females, with their mode of life, their want of exercise, and with certain peculiarities in their dress and education. Occasionally, however, we do meet with it in males of delicate constitution, or in those who have strained, or otherwise injured the spine.

The other form of spinal disease, I conceive, is quite peculiar to females. It is a most formidable and obstinate complaint, and depends most probably on the peculiar action of the

* Within these few weeks (January 1828,) I have had two cases of serious disease from spinal irritation in the Infirmary here, and one in private practice, in which the pain, shooting from behind the mastoid process over the scalp of one side of the head, appeared to be connected with spasmodic stiffness of the mastoid and other muscles of the neck of the same side. This connexion seemed to be quite decided in these cases of spinal irritation of the neck; but it is not so easy to ascertain whether or not partial spasm of the muscles, arranged along the spine, exists in cases where the dorsal or lumbar spine is irritated. I have not as yet paid attention to it in these situations; but from the analogy with similar affections of the cervical nerves, I conceive it probable that it does exist. An irregular practitioner in England is celebrated for his success in the treatment of these cases. His practice consists solely in long continued friction of the affected parts of the spine, joined to posture. Now, it is easy to conceive that such practice may have influence in abating a partial spasm of the muscles of the spine, and that the regular surgeon may improve his treatment of these complaints by following a similar plan.

uterine system. In one of the cases related, and in two at present (1827) in the Infirmary, the catamenia were suppressed for a long time, and in the former these complaints went off whenever the catamenia re-appeared, which was profusely. In most of the other examples, however, the catamenia were quite regular, so that there is no necessary connexion between amenorrhœa and this disease. Still, however, it is so purely a female disease, that I conceive some unknown change in the uterus must be present; perhaps a plethoric state of that viscus, with a similar condition of the blood-vessels of the spine.

There are several other diseases different from those which have been mentioned, in which also the spinal nerves are irritated. I shall take merely a short notice of these.

In painful affections of the throat, we often find that the cervical vertebræ are tender, and that there is a distinct connexion between the pain of the one and the tenderness of the other. When we touch the spine, the pain extends forward to the throat. This is remarkable in cynanche maligna, in which disease the patient often complains severely of the neck, and in unfavourable cases, there is great stiffness, as well as pain.

In several instances of ulcerated sore throat from lues venerea, or from mercury, I have seen this pain in the neck a very prominent symptom; and when the neck was touched, the pain extended forwards to the throat. In cases of this description, a blister applied to the nape of the neck is more effectual than to the throat. The propriety of applying a blister to the nape of the neck, in these cases, should probably be determined by the presence or absence of pain in that part. Where this is present, I have seen the most marked improvement follow the blister.

I have repeatedly seen a very painful affection extending from the 3d or 4th dorsal vertebra, forwards below the scapula, and down the arm. The pain is felt entirely in the arm, but when we press on the spine between the scapulæ, there is pain of the spine, distinctly connected with, and increasing the pain of the arm.

In one of these cases, H. L. (December 1823,) this pain totally prevented sleep for several nights. It affected the left mamma, and the whole of the left arm. When pressure was made near the spine, the pain extended through all the affected parts. Whenever a small blister near the spine began to rise, the pain was immediately removed, and did not return. I have seen many similar cases, in some of which percussion, applied near the spine, speedily removed the pain.

Pains in the spine, nearly allied to the subject of this essay, are frequently connected with the uterus, either during the state of menstruation or in diseases of that viscus. These are usually about the 2d or 3d lumbar vertebra. Pains in the back are well known to be frequent, during the monthly period in women, and we often at the same time observe severe spasmodic affections, in some part of

the abdomen, most usually of the arch of the colon or of the caput coli. I have often been able at once to trace these pains to irritation of the spinal nerves, by the usual examination of the spine. Immediate relief was generally attained by a blister of a minute size, or some stimulating application made to that part.

During my attendance on the Lock Hospital, I saw repeated instances of inflamed uterus, apparently connected with the peculiar profession of the patient, and in each of these, there was a very decided and marked affection of the spinal nerves. In these patients, the os uteri was prominent, hard, and acutely painful to the touch, and the connexion between this part, the spine, and the groins, where the round ligaments terminate, was rather singular. In the case of J. S., who had acute hysteritis, when the pained part of the spine was pressed on the right side, the pain extended to the right groin, when the left side was pressed to the left groin, and when the middle of the spine was touched, the pain was felt above the pubis, apparently in the uterus. In the other cases of hysteritis, this singular union was not looked for, but in the whole, there was a marked connexion between the spine, the uterus, and the groins. I found too that local means, especially blisters to the sacrum, afforded very marked benefit in abating the severity of the disease.

Some time ago, along with two medical friends, I visited a gentleman in whom there was pain around the anus, increased by going to stool, by motion, or by the slightest touch, so severe that he was fixed by it to one posture in bed. The pain could not be attributed to piles, or to any obvious cause. It was much increased by purging. It yielded in part to the application of leeches around the anus, but it completely subsided, whenever a blister applied to the sacrum began to act. It seems probable that the nerves arising from the sacrum were irritated in consequence of enlargement of the blood-vessels of the part, and that this state was removed by the leeches and blister.

I have observed in a great proportion of the numerous cases of painful and diseased liver that I have met with, that there was a distinct painful tenderness of the spine, about the 9th or 10th dorsal vertebra. Pressure there immediately excites pain in the region of the liver. This pain is almost always on the right side of the spine. There are a few exceptions from this general rule, but where there is much pain in the liver, there is generally also pain of the spine. Pain in the right side from spinal irritation alone, is often a source of error in the treatment of diseases of the liver and of the stomach. It is often mistaken for disease in the liver, and where it has been originally connected with a fault in that viscus, it may continue from spinal irritation alone, after the diseased state of the liver has been removed.

I have also repeatedly observed this pain of the spinal nerves, in cases where the stomach was much disordered, especially where it was

very acid. In such a case the pain shooting forward from the spine to the region of the liver, may give rise to the idea that the liver is diseased. We ought to recollect, however, that acidity in the stomach, or perhaps in the duodenum, is sufficient to occasion irritation of a spinal nerve, and that, as in other instances, the pain is felt at the extremity of this nerve, somewhat in the side, and often exactly in the right hypochondrium. The immediate cause of this pain is at once ascertained by the usual examination of the dorsal spine.

Reasoning on the various cases which we have detailed, in which distant pains and other morbid affections appeared to yield most readily to local means, applied to the spine, we should feel inclined to expect that in many complaints in the thorax and abdomen, where blisters and other local applications were indicated, we would produce a more powerful and direct influence, by applying these to the spine than to any other part. Blisters applied to the back are perhaps more inconvenient than to the breast or abdomen, but very probably they are more efficient.

The nerves which supply the viscera of the thorax, and of the abdomen, enter from behind, and a number of these proceed directly from the spine to the viscus to which they are distributed. Of course in inflammatory and other affections of these organs, blisters and other applications to the back are perhaps more efficient than we are aware of. At any rate, if in these diseases we were to discover any degree of tenderness in the spine, extending to the affected viscus, we would expect considerable benefit from a blister on the spot. The old-fashioned plasters on the back, are perhaps more useful than modern practitioners suppose.

I have now given a general account of those complaints, in which for some time past I have observed that the nerves of the spine were irritable. In a great proportion of these, some distant pain was distinctly occasioned by this cause; and in many of them this immediately subsided, by directing the attention to the spine. In several of them, however, the spinal nerves instead of being primarily affected, appeared to sympathize with some other organ, but in the whole of them, I am confident that the state of the spine was entitled to great attention, and that we did more good by local applications to this nervous centre, than to the pained part at a distance from it.

Treatment.—In the *simple* form of this disease, where the irritation is limited to one nerve, the recumbent posture for a few days, is frequently sufficient to remove the pain. If the uneasy weakness continue in spite of this, we ought to apply leeches, and probably a blister to the affected part of the spine. There is no advantage in applying a large blister. If the disease has been long fixed to one spot, it is likely that the ligaments of the part have become thickened, and that even a caustic issue on the spot, kept open for some time, be necessary.

For the removal of the more *formidable* dis-

ease, other measures are needed, and in fact we often find that a long time passes before there is much improvement.

If the disease be at all violent, the recumbent posture is absolutely necessary. It is quite obvious, indeed, that, without this, even the most active treatment will fail in producing any permanent effect, since it is only by removing pressure, and avoiding motion, that the nerves and muscles can be allowed to recover their powers. Besides, when the nerves are in an over-sensitive state, which is a frequent occurrence in severe cases of this disease, motion of any kind, by irritating the nerves, is apt to occasion, or increase, partial spasm of the neighbouring muscles, and these spasmodic contractions, by forcibly drawing the vertebrae to one side, increase the pressure on the too sensible nerves, and in this way the pain may become agonizing. Total rest, therefore, in severe cases, is absolutely necessary. Leeches should be freely applied, or blood taken by cupping the pained part of the spine.

After the pain has relaxed, I conceive that we ought to give a fair trial to long continued friction, having previously fomented the pained part. We may either rub the part with wheat flour, or starch powder, as is now so frequently practised in various local diseases; or we may employ the anodyne balsam, or some oily substance, combined with an aromatic, such as the oil of thyme. Perhaps there may be an indication to use a combination of the extract of belladonna with the oil or lard, for we know that this drug has a powerful effect in relaxing muscular fibres, and we have some reason to believe that partial spasm of the muscles arranged along the spine, has very material influence in occasioning pressure on the nerves as they issue from the spinal marrow.* We ought also to consider, that by long continued friction, we prevent the muscles of the

* An itinerant practitioner, already referred to, much celebrated for the success of his practice in cases of this description, directs the back to be well rubbed where there is any pain, for twenty minutes; the patient sitting with his back opposite a fire. The friction is employed morning and evening, *across* the spine from behind forward, not up and down the spine, as is usually done. He confines the friction to three parts of the spine, viz. to the upper part of the neck, the middle of the back, and the middle of the lumbar spine. He employs a nostrum of his own, which seems to be merely some oily substance combined with oil of thyme. This is probably a good stimulant, and ought to be more employed by regular medical practitioners. This same practitioner recommends a nourishing diet, with attention to the bowels, but he prescribes no internal medicine of any description. He leaves this entirely to the regular medical attendant of the patient. He orders the patient to lie on a firm mattress, in the horizontal posture, and occasionally to employ as much exercise as she can without exciting pain.

back from becoming weak, from want of exertion. The attempts at motion and exertion, however, must be made with considerable caution. They must be proportioned to the feelings of the patient.

It is an object, also, gradually to increase the slope of the sofa, according to the feelings of the patient, for by doing so, we give the muscles of the spine more room for exertion.

If these measures are not sufficient to remove this tedious and troublesome complaint, we must have recourse to other means. Blisters are often of use. They need not be large. If they merely cover the pained part, the size is quite sufficient. A small blister behind the ear or to the temple, is often effectual in removing severe pain, shooting over the head. I have found the antimonial irritation to be well suited for the removal of these spinal diseases. In two instances we found that obstinate disease subsided whenever erysipelas appeared on the affected part, and surely the antimonial eruption is much allied to this other inflammatory affection. We can apply an antimonial plaster to the pained part of the spine, or we may rub with the antimonial ointment morning and evening.

It is most likely, that the febrile excitement, which occasionally attends the eruption from antimony, may be of material service in removing the affection of the spine. In one well marked case in the Infirmary here (A. W.,) who had been a patient for a long time, with irritation of different portions of the spine, this disease went off completely, on being seized with typhus fever. She is at present (February 1828) convalescent from fever, and quite free of spinal irritation. If we had any means of exciting fever, we should perhaps remove the affection of the spinal nerves.

If these different means were to fail, I would have no hesitation in applying a blister to the head, even although no partial palsy or affection of the sight were present. If, however, there had been weakness of any part, or want of power, or intolerance of light, or dimness of sight, although there had not been any pain of the head, I would apply a blister to the head more early in the disease. I conceive that on many occasions my attention ought to have been more decidedly directed to the state of the head than it was; since I am now persuaded that in these diseases the brain is more frequently in fault than we are aware of.

In the treatment of this disease, it is of infinite consequence that the general health of our patient should be thoroughly attended to. The bowels of course must be properly regulated; the appetite improved by tonics, especially by the preparations of iron. Perhaps emmenagogues, with the hip-bath, if the catamenia are faulty, may be useful.

Minute doses of the blue pill may do good, if the biliary secretions be in fault, or if the urine be deficient; and, especially when joined with other diuretics, mercury may relieve vascular fulness in the head, by producing a flow of urine.

I conceive that I have seen the Prussic acid or valerian of use, in correcting excessive irritability of constitution.

The shower bath, or sponging with cold water and vinegar, are also occasionally proper. The clothing should be comfortable, and the diet nourishing.

Postscript.—It being some time since I wrote this long essay on a subject which I conceive to be of some interest, and which certainly has proved to me extremely puzzling and complicated, it may be useful to mention briefly my present impressions regarding its nature.

These are—that the immediate cause of the pain of the back and breast in spasm of one or other of the muscles, arranged along the spine, altering the position of the vertebræ, or otherwise compressing the nerves as they issue from the spinal marrow.

That this spasm, in many instances, is strictly a *local* disease produced by fatigue, wrong posture, or other causes, and quite unconnected with the state of the brain, spinal marrow, or nervous system in general.

But that, in other formidable instances, this partial, spasmodic, or wrong action of these muscles, is owing to a faulty state, perhaps an enlargement of the vessels of the brain, or spinal marrow. This state of the brain, as in many other diseases, gives rise to spasm, or even to convulsion of certain muscles; which partial symptom from its severity attracts the chief attention. This local affection is confined to those portions of the spine where there is the greatest motion, and where of course the muscles having the greatest activity, are most liable to deranged action or spasm. I imagine that this view of the subject is illustrated, and perhaps confirmed by various symptoms, which were observed in the different cases, and which without it, were very incomprehensible. The partial palsy, the affection of the sight, the giddiness of the head (for I find that this was a prominent symptom in several cases, especially in that of A. S.,) all give some confirmation to the notion that the brain is affected in these severe cases.

I have only to add farther, that if we pay attention to the number of the muscles, arranged along the spine, and to their functions, we shall see some reason for their being peculiarly liable to spasm. The variety of separate muscles in this situation is very great; and it ought to be kept in mind, that these are more constantly active than any other muscles, except the involuntary ones, since they are in a state of action in preserving the body in an erect state, as well as in every motion of the trunk.

This state of the muscles, as being the immediate cause of the pain, and of various uneasy feelings, is certainly entitled to marked attention, and we find it much moderated by posture, by local application, and especially by friction; but my conviction now is, that we ought to direct our attention at the same time to the state of the brain, as being the source on which severe cases of this description depend.

Glasgow, 13th February, 1828.

From the Archives Generales de Medecine.

RECHERCHES MEDICO-LEGALES POU-
VANT SERVIR A DETERMINER,
MEME LONG-TEMPS APRES LA
MORT, s'il y a eu empoisonnement, et à
faire connaître la nature de la substance vé-
néneuse. Par MM. ORFILA et A. LESSUEUR,
docteurs en Médecine. [Memoire lu à l'Aca-
démie de Médecine.]

In the greater number of cases, the physi-
cian charged with ascertaining the cause of
sudden death, is called before the inhumation
of the body; but it may also happen, that he
is not consulted for several days, and even
months afterwards. Is it possible to discover
the presence of a poisonous substance, by
subjecting to analysis the matters found in the
digestive canal of a body, ten, twelve, fifteen,
and eighteen months after its interment?
Upon questions of this nature, judicial tri-
bunals may demand information, and medical
jurisprudence does not yet possess any work
where that information may be found. We
cannot more forcibly illustrate the necessity of
these researches, than by alluding to the diffi-
culty experienced by the eminent gentlemen
whose opinion was requested in the case of
Castaing: those gentlemen were, MM. Vau-
quelin, Barruel, Chaussier, Laennec, Lermi-
nier, Magendie, Pelletan, Segalas, and Orfila.
Among other questions by the king's attor-
ney, upon several points of legal medicine,
relative to poisoning, it was demanded "whe-
ther the acetate of morphine, which had been
vainly sought for in the fluids arising from the
washings of the alimentary canal of Ballet,
might not have been decomposed by its mix-
ture with the putrid animal substances, the
more especially, that the proper chemical
researches were not instituted until several
days after death?" The commission, unable
to invoke experience upon this subject, and
guided only by analogy, after having long
agitated the question, were unanimously of
opinion, that the acetate of morphine might
be decomposed, and that decomposition would
affect at the same time, the acetic acid and
the morphine. We shall hereafter see that
this assertion is contrary to truth.

The solution of the problem under con-
sideration, has appeared to us to depend upon
two orders of experiments: 1st, poisonous
mineral and vegetable substances dissolved in
about a pint of water, in varying proportions,
were mixed with animal matters, and exposed
to the open air in large uncovered vessels,
during ten, fifteen, and eighteen months, care
being taken to renew the water as it evapo-
rated; 2d, the same substances, mixed with
albumen, gelatine, meat, &c., were introduced
into stomachs and intestines, and the whole
included in deal boxes, which were accurately
closed, and buried at a depth of two feet and
a half. Several months afterwards the boxes
were disinterred, and the substances contained
in the above mentioned viscera, analysed.

On the other hand, human bodies, buried in
deal coffins at a depth of four feet, were dis-

interred one or more months after inhumation,
in order to determine how long after death
traces of the digestive canal could be observed,
and the presence of poisonous substances, in-
troduced into the stomach or intestines during
life, demonstrated. These latter experiments,
which should be made the subject of a particu-
lar memoir, have taught us, that several months,
and even years after death, when none of the
soft parts are longer recognizable, there may
yet be found, upon the sides of the verte-
bral column, and in the abdominal region, a
kind of brownish mould, evidently the re-
mains of the alimentary canal, and in which
may exist a portion of the poisonous substance,
either changed, or in its natural condition.

The poisons which have been made the sub-
ject of experiment, are the sulphuric, nitric,
and arsenious acids, corrosive sublimate, the
acid tartrate of potass and antimony, the ace-
tate of lead, the proto-hydrochlorate of tin,
the sulphate of copper, the nitrate of silver,
the hydro-chlorate of gold, the acetate of mor-
phine, the hydro-chlorate of brucine, the ace-
tate of strychnine, opium, and cantharides.
We shall consider them in the order in which
they are mentioned.

Concentrated Sulphuric Acid.—On the 12th
March, 1826, six ounces of concentrated sul-
phuric acid, the fourth part of a human liver,
divided into small pieces, and a portion of in-
testine, were exposed to the air in a large
mouthed glass vessel. The 15th of the same
month, the contents were reduced into a kind
of pultaceous mass of a blackish brown colour,
exhaling a nauseous acid odour; it strongly
reddened litmus paper, and threw down, on
the addition of the hydro-chlorate of barytes,
an abundant precipitate of the white sulphate
of that mineral, insoluble in boiling water and
in nitric acid. Heated in a vial, with copper
in its metallic state, some time elapsed ere
sulphurous acid was disengaged, owing pro-
bably to the acid having been weakened by
the water contained in the animal substances;
by continuing the heat, however, a consider-
able quantity of this gas was obtained, and a
sulphate of copper was formed. On the 26th
May, 1827, twenty-two and a half months after
the commencement of the experiment, the
mixture appeared under the form of a black
pultaceous mass, possessing all the characters
above indicated; mercury, which was substi-
tuted for the copper, to disengage the sul-
phurous acid, was changed into a proto-sul-
phate. During the time intervening between
the two periods just mentioned, the contents
were examined at least twenty times, and al-
ways with the same result.

Dilute Sulphuric Acid.—July 18th, 1826,
twenty grains of concentrated sulphuric acid, a
pint and a half of water, and about the third of
an intestine, taken from a human subject, were
introduced into a vessel similar to that em-
ployed in the first experiment. On the 12th
of the following August, the liquor was of a
yellowish white colour, *strongly reddened* an
infusion of litmus, and, treated by the soluble
salts of barytes, threw down a white precipi-

tate insoluble in water and nitric acid. We wished to ascertain whether by boiling the liquid with mercury, sulphurous acid might be obtained, but by reason of the large quantity of animal matter it contained, it foamed and evaporated ere the gas was perceptible. May 21st, 1827, nine months and three days after the commencement of the experiment, the mixture exhaled an insupportable odour; diluted with water and filtered, it *scarcely reddened* litmus paper; the greater part of the sulphuric acid having been saturated by the ammonia evolved during the putrefactive process; the sulphate of ammonia was decomposed, and the ammonia evolved in great quantity, when the liquid was boiled with quicklime. With the barytic salts it afforded a copious white precipitate of the sulphate of barytes, insoluble in water and nitric acid; concentrated by evaporation and boiled with mercury, it gave out no sulphurous acid gas, notwithstanding it was reduced almost to a state of aridity. Perceiving that it was impossible by this method to prove the identity of the free acid of the liquor with sulphuric acid, the following process was instituted. A portion of the liquor was heated at a low temperature with *pure* subcarbonate of lime, which had been previously boiled in distilled water, and did not contain a particle of the sulphate; there was no effervescence; after keeping it in a state of agitation about ten minutes, it was filtered. The white mass left upon the filter, washed with distilled water to remove any sulphuric acid and sulphate of ammonia which it might contain, was dried and treated in a vial with boiling distilled water. The filtered solution was not disturbed by the hydro-chlorate of barytes, nor by the oxalate of ammonia, and therefore did not contain sulphate of lime. It is evident, then, that the free sulphuric acid which existed in this liquor, was so weak as scarcely to form any sulphate of lime, and the little which was produced, was dissolved in the water employed in washing the precipitate.

One drachm of *concentrated sulphuric acid* was introduced on the 10th November, 1826, with a portion of intestine, into a porcelain vessel, which was enclosed in a deal box, and buried at a depth of two feet and a half. It was disinterred on the 30th April, 1828, seventeen months and twenty days after the inhumation. The intestine had a slightly yellowish tint, and appeared to swim in a grayish and somewhat turbid liquid, which reddened litmus paper, effervesced when thrown upon the pavement, with the salts of barytes threw down a white precipitate insoluble in water and nitric acid, and when boiled with mercury, gave out sulphurous acid gas; it contained therefore free sulphuric acid; it was necessary, however, in order to ascertain its existence, to prolong the ebullition almost to dryness, probably owing to the acid having been much weakened by the humidity of the intestines.

It is proved therefore, 1st, that it is possible to determine the presence of *concentrated sul-*

phuric acid, several months, and even years after its admixture with animal substances; 2d, that if this acid have been *greatly diluted*, and mixed with substances which, in undergoing decomposition, evolve a large quantity of ammonia, it is saturated by this alkali, so that none, or very little of the acid remains in an uncombined state after the lapse of a few months; 3d, that in this case, the circumstance of poisoning by sulphuric acid cannot be proved; that however, from the existence of the sulphate of ammonia, which we suppose to have been obtained in a crystalline and well characterized state, there will be a presumption that such has been the case, as this compound does not ordinarily form a part of the aliment, nor enter into the composition of the tissues belonging to the digestive canal; 4th, that if there should still remain a quantity of free sulphuric acid, the best method of determining unequivocally its existence, would be to treat the liquor with *pure* sub-carbonate of lime, when sulphate of lime will be formed at the expiration of a few seconds; while the sulphate of ammonia, treated with the sub-carbonate at the ordinary temperature, is not decomposed till after a longer time.

Concentrated Nitric Acid.—On the 12th March, 1826, ten ounces of the nitric acid of commerce, and portions of intestine and liver taken from a human subject, and divided into small pieces, were introduced into a large mouthed glass vessel, and exposed to the air. The 19th of the same month the mixture had a yellow colour; the transparent liquid strongly reddened litmus paper, and *did not act* upon copper, either at the ordinary temperature or when raised to *ebullition*. To obtain the orange coloured nitric acid gas, it was necessary to evaporate the liquor to dryness, and decompose, by means of heat, the nitrate of copper which had been formed. Mixed with a small quantity of solid potash prepared with alcohol, it became red immediately, and furnished by evaporation a residuum of the same colour, which fused, like nitrate of potash, when heated with charcoal, and placed in contact with copper, sulphuric acid, and a few drops of water immediately furnished orange coloured nitrous acid gas. May 26, 1827, fourteen months and a half from the commencement of the experiment, the liquor treated with solid potash, ignited charcoal, copper and sulphuric acid, in the manner just mentioned, gave the same results.

Dilute Nitric Acid.—On the 18th July, 1826, a vessel like those employed in the former experiments, containing a pint and a half of water, twenty grains of nitric acid, and nearly a third of an adult intestine, was exposed under similar circumstances. The 12th of the following August, the fluid had a yellowish tinge, *reddened litmus paper*, and, evaporated to dryness with caustic potash, left a reddish residuum, which, thrown upon ignited charcoal, did not fuse, but became carbonized, exhaled an odour like burning horn, and, in a word, comported itself as a substance rich in animal matter; treated with

copper and sulphuric acid, it effervesced, but the odour and colour of nitrous acid gas, were with difficulty distinguishable. May 23d, 1827, six months and four days from the commencement of the experiment, the mixture exhaled a very fœtid odour: the filtered liquor, far from reddening litmus paper, *changed to a blue* the colour of paper reddened by the action of an acid, which was due to the presence of a quantity of ammonia; treated with potash at a boiling temperature, much ammoniacal gas was disengaged, and nitrate of potash was formed; since, when evaporated to dryness, and the residuum agitated a short time with distilled water, a liquid was obtained, which being filtered and evaporated, furnished a salt, with potash for its base, which fused upon ignited charcoal, and gave out nitrous acid gas when mixed with copper, sulphuric acid, and a little water.

A drachm of *concentrated nitric acid* having been placed, with a portion of intestine, in a small porcelain vessel, was enclosed in a deal box, and buried at a depth of two feet and a half, on the 10th November, 1826. It was disinterred on the 30th April, 1828, seventeen months and twenty days afterwards; the intestine had not assumed a yellow colour; in the vessel was found about three drachms of a turbid, grayish liquid, which reddened litmus paper, effervesced when thrown upon the pavement, did not act upon copper at the ordinary temperature, and which, saturated with potash and evaporated to dryness, left a residuum that fused when thrown upon ignited charcoal like the nitrate of potash, and disengaged vapours of orange coloured nitrous acid, when mixed with copper, and treated with sulphuric acid diluted with a small quantity of water. It contained therefore nitric acid.

These experiments prove incontestibly, 1st, that the presence of concentrated nitric acid may be demonstrated several months after its admixture with animal substances, and when the putrefactive process has arrived at its maximum; 2d, that for this purpose, potash is preferable in the first instance, to metallic copper; 3d, that the acid cannot be detected when it has been considerably diluted with water, and employed in small quantity—the ammonia which results from the putrefaction of the animal substances, being sufficient to saturate all the acid; 4th, that in this case we cannot do more than establish the presence of the nitrate of ammonia, which does not necessarily infer poisoning by nitric acid, since this nitrate might possibly have been formed during the putrefaction of the animal substances.*

Arsenious Acid.—March 8th, 1826, a glass

* We may observe, however, that having macerated an intestine in distilled water, from the 27th February until the 23d of April following, there was no formation of nitrate of ammonia, notwithstanding the vessel was constantly exposed to the air, and the putrefactive process had reached its height.

vessel, containing a pint and a half of water, holding in solution three drachms of arsenious acid, and pieces of muscle, brain, and intestine, was exposed to the air as in the former experiments. The contents were examined on the second day of the following August, nearly five months afterwards, and *exhaled no disagreeable odour*; the filtered liquor, treated by the hydro-sulphuric acid, by the ammoniacal sulphate of copper and by lime water, furnished results similar to those obtained from a solution of arsenious acid in pure water.

Arsenious Acid, much more diluted with Water.—Six grains of arsenious acid, dissolved in a pint and a half of water, were exposed, July 18th, 1826, in a glass vessel, with about one-third of an adult intestine. On the 12th of August following, the mixture *scarcely exhaled* any disagreeable odour; the filtered liquor neither assumed a yellow hue, nor threw down a precipitate on the addition of hydro-sulphuric acid; the sulphate of copper occasioned no change; during evaporation there was a considerable coagulation of animal matter, which was removed as it formed; the product of evaporation, treated with boiling distilled water for the space of four or five minutes, contained arsenious acid, since the solution reddened by the action of hydro-sulphuric acid, and on the addition of one drop of hydro-chloric acid, threw down a precipitate of the yellow sulphuret of arsenic, soluble in ammonia. The yellow colour, and precipitate, resulting from the action of the hydro-sulphuric acid, were much less sensible, when, instead of operating in the manner above mentioned, the reagent was added to the liquid simply heated to the boiling point, and filtered, in order to coagulate the animal matter. On the 5th of May, 1827, nine months and a half from the commencement of the experiment, the mixture exhaled a fœtid odour; it filtered with difficulty, by reason of the great quantity of animal matter it contained in solution; it *rapidly* changed to a blue, the colour of litmus paper which had been reddened by exposure to an acid; hydro-sulphuric acid, and the ammoniacal sulphate of copper, occasioned *no alteration*, although they demonstrated the presence of arsenious acid when, after being evaporated to dryness in order to separate the animal matter, the residuum was treated with boiling distilled water.

The same experiment, repeated on the 27th of February, and the liquid examined on the 27th of April following, furnished analogous results.*

* We wish particularly to call the attention of the reader to the fact, that by mixture with animal matter in a state of solution, arsenious acid may be *masked* so completely, as not even to develop a yellow colour by the addition of hydro-sulphuric acid; but we may also remark, that in these cases, it is only necessary to evaporate the liquor to dryness, and dissolve the product in boiling water, to obtain a solution, in which the hydro-sulphuric, mixed

Solid Arsenious Acid.—November 8th, 1826, the white of an egg, portions of meat, bread, and twenty grains of solid arsenious acid, were introduced into a portion of large intestine taken from an adult, and the whole included in a small deal box, which, after having been perfectly closed, was buried at a depth of two feet. August 14th of the following year, nine months and six days afterwards, the box was disinterred, and the contents of the intestine agitated in distilled water; after the lapse of a few minutes, it was filtered, and the addition of hydro-sulphuric acid evinced the presence of a large quantity of arsenious acid.

Having sprinkled two thick slices of the lean of veal with arsenious acid, M. Dubuc enclosed them in a strong oaken box, which he buried in a soil sufficiently permeable to water. Upon exhumation about six years afterwards, a kind of mould was found, which crumbled beneath the fingers, and contained so much arsenic, that 24 grains thrown upon ignited charcoal, infected, by its alliaceous odour, the air of a large laboratory. (*Journal de Chimie Médicales*, tom. ii. p. 278.)

It follows from the preceding facts, 1st, that the presence of arsenic which has been mixed with animal matters, may be detected even after the lapse of several years; 2d, that it is nevertheless necessary in many cases to remove a part of these matters, which is readily effected by evaporating to dryness, and agitating the product for several minutes with distilled water; 3d, that if the arsenious acid have been employed in the solid state, it will be sometimes possible, even long after inhumation, to discover here and there particles of the mineral, which present all the characters of this poison; 4th, it is indubitable, that in proportion as ammonia is disengaged, the arsenious acid is transformed into an arsenite of ammonia, which is *much more soluble* than arsenious acid; so that it may happen, that

with a drop of hydro-chloric acid, will precipitate all the arsenious acid in form of the yellow sulphuret. This important fact, the correctness of which, one of us has already several times had occasion to verify before judicial tribunals, in cases of poisoning by arsenious acid proved a few days after death, evinces how much the difficulties attending the discovery of this poison when mixed with animal substances, have been exaggerated. If writers, who have given much more complicated methods than that which we propose, have not found the arsenious acid in the liquids ejected by vomiting, and those contained in the stomach and intestines, it is because they have not followed exactly our footsteps, and especially because they have not made use of the hydro-sulphuric and hydro-chloric acids as reagents, but have employed the deuto-sulphate of ammoniacal copper, which is an unfaithful test, as we have already elsewhere established. (Vide Orfila, *Leçons de Médecine-légale*, tom. iii. pag. 112, deuxième édition.)

after a lapse of several years, we may not be able to demonstrate the presence of arsenious acid, when it would have been easy to have done so, some months after inhumation; because this acid, once transformed into the arsenite of ammonia, will have become soluble, and have filtered into the earth through foramina, which are often found in the parietes of the coffin, when putrefaction has made great progress; 5th, that if arsenious acid, when largely employed, arrests the putrefaction of animal substances, such is not the case when the quantity is very small.

Corrosive Sublimate.—March 8th, 1826, three drachms of corrosive sublimate dissolved in two ounces of boiling water, were introduced into a large mouthed glass vessel containing two pints and a half of water, together with meat, cerebral matter, and portions of intestine. On the nineteenth, the mixture *did not exhale a fœtid odour*; the animal substances were indurated, and as if tanned; the filtered liquor hardly assumed a brown colour by the action of hydro-sulphuric acid; potash and ammonia, at most, only rendered it opaline, but a plate of gold, covered in a spiral form by a leaf of tin, was immediately covered with a layer of metallic mercury upon being plunged into the liquid, to which a few drops of hydro-chloric acid were at the same time added. From the animal substances, washed and well dried, metallic mercury was obtained by calcination, in a retort or small glass tube. Examined on the 18th June, 1827, the results were precisely similar.

On the 18th April, 1826, half of the liquid under consideration, and which contained so small a quantity of the sublimate, that the existence of the latter could only be detected by means of a plate of gold, was mixed with other animal substances, (liver, spleen, and intestines.) On the 20th of the same month, the mixture exhaled an excessively fœtid odour, and the addition of potash, ammonia, and hydro-sulphuric acid, occasioned no change of colour in the liquor. The plate of gold, employed as above mentioned, was not whitened, even after the lapse of an hour.

Corrosive Sublimate diluted with a large quantity of Water.—On the 18th of July, 1826, six grains of corrosive sublimate, together with a portion of intestine, were introduced into a glass vessel containing a pint and a half of water. The mixture, examined on the 2d of August following, exhaled a *very fœtid odour*; hydro-sulphuric acid, the hydro-sulphates, potash and ammonia, neither threw down any precipitate from the filtered liquor, nor rendered it cloudy; a plate of gold immersed into it, did not assume a white colour till after several hours. The intestine, dried and calcined with potash, furnished metallic mercury.

These experiments lead us to the conclusion, 1st, that corrosive sublimate, dissolved in water, is decomposed by animal substances with sufficient rapidity to render it impossible, after some days, to demonstrate its presence otherwise than by a plate of gold covered spirally by a leaf of tin, and assisted by the

action of hydro-chloric acid; 2d, that the quantity of the sublimate decomposed, is proportionate to that of the animal matter employed; 3d, it would appear, however, that the animal matter was not capable of decomposing the whole of the sublimate, since after the lapse of several hours, *an atom* of metallic mercury was obtained from a solution of six grains of the sublimate, mixed with a *large quantity* of animal matter; 4th, that in all these cases, by subjecting the animal matter to the action of charcoal, assisted by an elevated temperature, metallic mercury may be obtained even after an interval of several years. Now, the presence of this metal, if it do not prove the existence of corrosive sublimate, at least evinces that of some mercurial preparation.

Acid Tartrate of Potash and Antimony.—Three drachms of tartarised antimony, dissolved in two pints of water, the fourth of a human liver, and a portion of intestine, were exposed to the air in a wide mouthed glass vessel, March 29, 1826. The mixture was found in a putrid condition on the ninth of the following April; the filtered liquor, treated by hydro-sulphuric and sulphuric acids, lime water, and an infusion of galls, furnished the same results as a solution of emetic tartar. April 28th, hydro-sulphuric acid and the hydro-sulphates, no longer threw down any precipitate, proving that the liquor did not contain tartarized antimony, or rather, that if it were present, the animal matter held in solution, prevented its detection by those reagents; sulphuric acid, and the infusion of galls, produced a grayish white precipitate, evidently the product of the action of these agents upon the animal matter held in solution.

The liquor being filtered and evaporated to dryness at a gentle heat, a residuum was obtained, which, by agitation for a few minutes with warm distilled water, formed a solution, from which a precipitate of the hydro-sulphate of antimony was thrown down, on the addition of hydro-sulphuric acid. The sixth of June of the same year, the liquor no longer contained antimony, no change resulting from the action of hydro-sulphuric acid, even after it had been evaporated, and the residue treated with water; but metallic antimony was obtained from the animal matters, after they had been dried and sufficiently calcined.

Tartrate of Potash and Antimony diluted with a large quantity of Water.—July 18th, 1826, six grains of tartarized antimony, dissolved in a pint and a half of water, and about the third part of an intestine, were exposed together in a glass vessel. Examined on the second of August, the hydro-sulphuric acid and the hydro-sulphates, occasioned no disturbance in the liquor. The solid matter, upon being dried and calcined, furnished metallic antimony.

It follows from the preceding facts, 1st, that tartarized antimony, when mixed with animal matter, is decomposed after the lapse of a few days, the tartaric acid being destroyed,

and an oxide of antimony precipitated; 2d, that it is then impossible to demonstrate its presence in the liquor, by the tests ordinarily used to detect the salts of antimony, but that this metal in its metallic state may be extracted from the solid substances, even after the lapse of several months; 3d, that the decomposition is rather the result of the action of air and water upon the salt, than of the animal substances, for it has been proved by experiment, that a solution of three drachms of tartarized antimony in a pint and a half of distilled water, undergoes similar decomposition when exposed to the air, and that the antimonial salt cannot be detected in it after a period, varying in summer from thirty to forty days, any more than it could in a similar solution, to which albumen and gelatine had been added.

Acetate of Lead.—March 29th, 1826, two pints of water, holding in solution three drachms of the acetate of lead, were exposed in a large mouthed vessel, with muscle, liver, and portions of intestine. On the ninth of April following, the solution no longer contained acetate of lead, for when filtered, it did not change its colour on the addition of hydro-sulphuric acid; but lead in its metallic state, was obtained by drying and strongly calcining a blackish gray precipitate which had been thrown down, and the animal substances above mentioned.

Acetate of Lead greatly diluted with Water.—Six grains of the acetate of lead, in a pint and a half of water, were exposed, with about the third part of an intestine, July 18th, 1826. Four days afterwards, no trace of the salt could be discovered in the solution, but the solid matters being calcined, yielded a sensible quantity of lead.

It is evident, therefore, that we should not search in the liquid for the acetate of lead, which after solution has been placed in contact with animal matter; and it appears moreover, that a very short time is required to effect its decomposition.

Proto-hydrochlorate of Tin.—On the 10th July, 1826, two drachms of this salt, dissolved in a pint and a half of water, and the third part of an intestine, were exposed under the same circumstances as in the preceding experiments. The mixture exhaled a very fetid odour, when examined on the second of the following August. The liquid, filtered, and treated with hydro-sulphuric acid and the hydro-sulphates, remained colourless, while by drying separately, and calcining the intestine and a flocculent grayish matter which had precipitated, metallic tin was obtained; from which it follows, that a very short time is required for the entire decomposition of an aqueous solution of the proto-hydrochlorate of tin by animal matter.

Sulphate of Copper.—March 12th, 1826, an intestine, plunged into a solution of three drachms of the deuto-sulphate of copper in two pints of water, was exposed to the air in a wide mouthed glass vessel. On the 18th of the following June, the mixture gave out an excessively fetid odour; the filtered liquid

had a dirty bluish green colour, and threw down a chesnut brown precipitate when treated by the ferro-prussiate of potash, and a black precipitate, by the soluble hydro-sulphates; it changed to a blue, on the addition of ammonia. Desirous to ascertain to what point the solution retained all the sulphate of copper which had been added, a portion was diluted with fifteen times its volume of water, and it was ascertained that it was scarcely affected by the above mentioned reagents, while another portion of the same solution, which had been set apart *prior to its admixture with the intestine*, instantly afforded a precipitate, though diluted with two hundred times its volume of water. It became therefore indispensable to ascertain whether the oxide of copper, which appeared to have separated from the solution, was not contained in the solid matter. The latter, after being carefully washed to remove any sulphate of copper which might be mixed with it, was dried and calcined; the residuum, besides presenting here and there reddish particles of metallic copper, furnished, when treated by hot nitric acid, a perceptible quantity of the nitrate of copper.

Sulphate of Copper greatly diluted with Water.—On the 18th July, 1826, six grains of the deuto-sulphate of copper, dissolved in a pint and a half of water, were exposed in a vessel containing intestine. August 2d, the mixture exhaled a very fœtid odour, the liquor was *nearly colourless*, and contained none of the cupreous salt, its colour remaining unchanged on the addition of the ferro-prussiate of potash, ammonia, and hydro-sulphuric acid. Nitrate of copper was obtained by treating the carbon, resulting from the calcination of the intestine, with nitric acid.

These experiments prove, 1st, that by admixture with animal matter, the deuto-sulphate of copper is decomposed, so that after the lapse of a certain time, none of the salt remains in solution; 2d, that however, this decomposition is not so rapid, that a portion of the salt may not be found in solution, even after several months, if the experiment be made on several drachms of the salt; 3d, that in all cases in which the cupreous salt cannot be discovered in the liquor, the solid substances should be dried and calcined, in order to extract the metallic copper, while a portion of the carbon should be subjected to the action of nitric acid, that a nitrate of copper may be formed.

Verdigris.—November 8th, 1826, a stomach, containing twelve grains of verdigris, pieces of meat, the white of an egg, and a quantity of *soupe maigre*, was enclosed in a thin deal box, and buried at a depth of two feet and a half. It was disinterred on the seventh of August, 1827. The contents of the stomach were green; after they had been cut into small fragments and boiled in distilled water, the filtered liquor afforded, with the usual tests, no indications of the salts of copper; this was also the case with the liquor obtained from boiling the stomach in water. Dilute hydro-

chloric acid being placed in contact with all the green portions, the latter assumed an *unctuous and grayish aspect*; after being agitated for a few minutes, the solution was filtered; it was of a greenish blue colour, and afforded a chesnut brown, a black, and a blue precipitate, when treated respectively by the ferro-prussiate of potash, hydro-sulphuric acid, potash, and soda; ammonia imparted to it a blue colour, as it does to the salts of copper. It follows from what has been stated, 1st, that by its sojourn with animal matter under ground, verdigris is decomposed, and the deuto-oxide of copper forms with the fat of the dead body a kind of soapy matter, insoluble in water; 2d, that in a case of poisoning by this substance, it would be possible to demonstrate the presence of the deuto-oxide of copper, by means of hydro-chloric acid and calcination, several months, and even years, after inhumation.

Nitrate of Silver.—July 12th, 1826, a drachm of the nitrate of silver, dissolved in a pint and a half of distilled water, was exposed, as in the former experiments, with a portion of intestine. When examined on the second day of the following month, the mixture had already become very fœtid; the colour of the filtered liquor underwent no change by the addition of hydro-sulphuric acid, and was scarcely disturbed by the hydro-chloric acid and its salts. Upon drying and calcining separately the intestine, and a flocculent brownish precipitate which had been formed, metallic silver was obtained. Nitrate of silver, in solution, is then rapidly and completely decomposed by animal substances; so that, if called to give an opinion in a case of poisoning by this salt, several days after inhumation, it would probably be necessary to endeavour to extract the metal from the solid matters.

Hydro-chlorate of Gold.—July 10th, 1826, pieces of liver, intestine, and a pint of water holding in solution thirty-six grains of the hydro-chlorate of gold, were exposed to the air in a wide mouthed glass vessel. On the second of August, the mixture had become very fœtid; the filtered liquor, treated by the hydro-sulphuric acid, the hydro-sulphates, and ammonia, afforded no trace of the salt; gold however was obtained by calcination of the solid substances; in fact these substances, dried and reduced to carbon by heat, gave a yellowish solution when treated by aqua regalis, from which the proto-hydrochlorate of tin threw down a purple, ammonia, a yellow, and hydro-sulphuric acid, and the proto-sulphate of iron, a brown precipitate; the same result was obtained from a grayish precipitate which had formed, and was carefully separated from the intestines in order to be calcined. The carbon, moreover, resulting from these calcinations, presented here and there brilliant reddish particles, which were evidently metallic gold. We shall say then, in relation to poisoning by the hydro-chlorate of gold, what we have already established when speaking of the nitrate of silver.

Acetate of Morphine.—A drachm and a half

of the acetate of morphine, dissolved in a pint of water, was mixed in a large mouthed vessel with *soupe maigre*, rich broth, fat, and several portions of intestine, and the whole exposed to the air on the 8th of March, 1826. Examined on the 26th of the same month, the mixture already exhaled a very fœtid odour; ammonia threw down from the filtered liquor, a grayish white precipitate; evaporated to dryness, a yellowish product was obtained, which changed to a *beautiful red colour* by the action of nitric acid, and to a blue, when treated with a slightly acidulous trito-hydrochlorate of iron; this latter shade, however, was less intense than that produced by the same reagent, upon a quantity of morphine equal to that of the product employed; there were, moreover, several *greenish spots* scattered here and there, the consequence of the admixture of the blue colour above mentioned, with the yellow tint of the product. The 9th of April following, the filtered liquor threw down a grayish white precipitate by ammonia, and furnished by evaporation a product which strongly *reddened* by nitric acid, and changed to a *green* by the trito salt of iron; in strictness, this green colour had a slight shade of *blue*, and afterwards of *brown*. On the 16th of April, the same characters were presented, with the difference, that the salt of iron produced with the residuum of evaporation an *olive green colour*, without any bluish shade. Similar results were obtained on the 18th of June, when the putrefactive process had arrived at its height.*

A portion of the liquor was filtered, on the 1st of August, 1826, and treated with ammonia, which threw down a brownish gray precipitate of *morphine*; in fact, by treating this precipitate by nitric acid, and rendering the solution colourless by the action of animal charcoal, a solid grayish product was obtained by evaporation, which nitric acid changed to a red, and the hydro-chlorate of the trito-oxide of iron, to a greenish blue colour. Another portion of the liquor, evaporated to dryness, yielded a yellowish brown residuum, which was treated with boiling alcohol; the alcoholic solution was evaporated to dryness, and the product treated by distilled water, and subsequently by the sub-acetate of lead, by hydro-sulphuric acid, and by purified animal charcoal, as advised by M. Lassaigne,—a liquid

* Fearing lest the beautiful blue colour, developed by the action of nitric acid upon the product of evaporation, might be the result of its action upon the putrid animal matter, rather than upon the acetate of morphine, we evaporated to dryness an excessively fœtid liquor containing none of the acetate, and ascertained that the product of evaporation became *only yellow* when treated with nitric acid. The liquor in question was obtained by exposing in an open vessel, from March 8th till the 18th of June following, a pint of water, mixed with soup, broth, fat, and intestine.

was obtained, which, evaporated in a sand bath, left a slight residuum of a yellowish white colour, which assumed a *very beautiful red* under the action of nitric acid, and a *greenish blue* under that of the trito-hydrochlorate of iron.

May 18th, 1827, fourteen months and six days from the commencement of the experiment, the liquor was excessively fœtid and strongly alkaline, restoring instantly the blue colour of litmus paper which had been reddened by an acid; about six ounces remained, the greater part having been employed in the several essays already described;* this liquor was divided into two portions, A and B. The portion A was evaporated, and treated successively by alcohol, the sub-acetate of lead, hydro-sulphuric acid, and animal charcoal, according to the method of M. Lassaigne; a solid product was obtained, slightly *yellowish*, which *reddened* by nitric acid, and assumed under the action of the hydro-chlorate of the trito-oxide of iron, a *red* or *brown* instead of a *blue* colour; this solid product, treated with distilled water at the ordinary temperature, was not entirely soluble, the portion dissolved, filtered, and evaporated to dryness, *reddened* under the action of nitric acid and the *salt of iron*, when it should have been changed to a *blue* by the latter test; the portion remaining on the filter, also *reddened* when treated with nitric acid, and became *blue* when the salt of iron was employed. The portion B, of the liquor, instead of being treated according to the process of M. Lassaigne, was simply filtered and evaporated to dryness; the product, which was of a *very brown colour*, was boiled several minutes in concentrated alcohol, to which it imparted its deep brown hue; the solution was treated with animal charcoal *purified* by means of hydro-chloric acid, and carefully washed, it was then filtered several times through another portion of charcoal, till it became nearly colourless; evaporated in a sand bath it left a yellowish product which strongly *reddened* on the addition of nitric acid, and became *blue* when treated with the salt of iron in solution, not however, unless the latter were employed in very minute quantity, as otherwise it produced a reddish colour. The result furnished by the portion B, compared with that obtained from A, proves evidently, that the sub-acetate of lead and hydro-sulphuric acid may be advantageously omitted, when endeavouring to detect the presence of morphine.

Acetate of Morphine diluted with Water.—On the 18th of July, 1826, six grains of the acetate of morphine, dissolved in a pint and a half of water, were exposed with about the third of an intestine, as already mentioned. The putrefactive process was at its height when examined on the 21st May, 1827, six months and three days from the commencement of the experiment. The liquor having

* It is almost unnecessary to state that water was added, in proportion as it was dissipated by evaporation.

been filtered, and evaporated at a gentle heat, the product, which was of a brown colour inclining to black, was treated with boiling alcohol; the alcoholic solution, evaporated to dryness, furnished a residuum which was subjected to the action of distilled water, sharpened with acetic acid. This new solution was rendered colourless by animal charcoal, and again evaporated to dryness; the product, which possessed a bitter taste, *reddened* by the action of nitric acid, but did not become *blue* when treated by the trito-hydrochlorate of iron; this test also imparted to it a reddish colour.

These experiments establishing satisfactorily, that the morphine was not destroyed, even several months after the admixture of its acetate with animal matter, we wished to ascertain what would take place in an aqueous solution of this salt exposed to the air, and it was observed that the acetate was *partly decomposed*, that the *acetic acid* of the decomposed portion was destroyed, while the *morphine of the same portion* was precipitated, if not entirely, at least the greater part. The following facts place beyond a doubt, the truth of these assertions.

1st. A drachm and a half of the acetate of morphine was dissolved in two pints of water. After ten months' exposure to the air, the liquor, which had long been covered with mouldiness, was turbid, and floated above a copious precipitate; filtered, and evaporated to dryness, it yielded a yellowish powder, which assumed a red, and a blue, colour when treated respectively, by nitric acid, and the salt of iron above mentioned. The precipitate which remained upon the filter, after having been washed several times with boiling water to remove any soluble matter which it might contain, was treated by boiling alcohol; the solution thus obtained being evaporated, deposited a notable quantity of crystals of *morphine*.

2d. May, 19th, 1827, twenty-four grains of the acetate of morphine were dissolved in a pint of distilled water; the filtered and *transparent* liquor, slightly reddened litmus paper, and was abandoned to the air in a large mouthed vessel. Eight days afterwards, floculi of mould were observed in the liquor; they had become much more numerous when examined on the third of August, though the liquor still retained its transparency. It restored the blue colour of litmus paper, had no sensible odour, and when a feather moistened with hydro-chloric acid was brought near its surface, gave out no white vapours, which would have been formed had there been any disengagement of ammonia. On the 27th of February, 1828, the liquor was turbid, and the sides of the vessel lined with yellowish and strongly adherent crystals. It had a yellowish amber colour when filtered, and after being evaporated to dryness, yielded a yellowish gray product, which nitric acid changed to a *red*, and the salt of iron to a *blue* colour; it was almost entirely soluble in distilled water, and appeared to consist of the acetate of mor-

phine, mixed with a very little foreign matter. The mould and other flocculent matters which remained upon the filter, yielded a similarly coloured residuum, which evinced the same phenomena when treated with the reagents just mentioned. After repeated boiling in distilled water, to remove any soluble matter they might contain, they were dried and boiled in alcohol of 38°, which dissolved only a part; the solution slowly restored the blue colour of litmus paper, and furnished on evaporation crystals of *morphine*. The matter adhering to the sides and bottom of the vessel, having been detached, and exhausted by means of boiling water, was dried, and boiled with alcohol of 40°, which dissolved it almost entirely; the solution was slightly alkaline, and yielded by evaporation a considerable quantity of perfectly crystallized *morphine*.

This decomposition of the acetate of morphine in water, has been likewise observed by M. Dublanc; and Geiger had already remarked, that it underwent an analogous decomposition in alcohol; but as stated by M. Dublanc, this spontaneous decomposition has its limits, and may be prevented by maintaining the liquor in an acid state. (*Journal de Pharmacie*, 1827, p. 264.)

It follows from all these facts, 1st, that in a case of juridical exhumation, it is possible to detect, several months after death, the presence of the acetate of morphine, or of morphine in the alimentary canal of an individual poisoned by a preparation of this nature; 2d, that for this purpose it is necessary to operate not only upon the liquids, but also upon the suspected solid matters, because if the poisoning had been effected by an aqueous solution of the acetate, this will have been decomposed, and the morphine in part precipitated; 3d, that there will be a smaller quantity of morphine precipitated, than might be at first view imagined, a part being re-dissolved by the ammonia formed during putrefaction; 4th, that to obtain the morphine which may exist in the solid matters, it is necessary in the first instance, to treat them repeatedly with alcohol, evaporate the solutions, and to act upon the residuum with water sharpened by acetic acid; without this precaution, it would be difficult to separate the morphine from the fat which is formed *abundantly*, during the sojourn of the body under ground; that the liquor may, if necessary, be rendered colourless, by heating it with *purified* animal charcoal, and repeated filtration through the same substance, without resorting to the sub-acetate of lead and hydro-sulphuric acid, the use of which has appeared to us to be at least useless; 5th, that it is apparent, in comparing the action of the nitric acid and of the trito-hydrochlorate of iron, upon the substances which were the object of the preceding experiments, that they were *invariably* reddened by the former, even when slightly discoloured, while the latter communicated a *blue colour*, in general, only when they were entirely colourless, and in certain cases imparted a reddish tint, even

when they were so; 6th, that in a case of juridical exhumation, it would be rashness to pronounce *affirmatively* that the person had been poisoned by a preparation of morphine, *merely* from observing the *red* and *blue* colour of which we have just spoken; that these phenomena afford, at most, only a slight presumption that such has been the case; 7th, that there will be no room for doubt if crystallized morphine be obtained, insoluble in water and ether, soluble in alcohol and nitric acid, fusible at a gentle heat, *reddening* on the addition of nitric acid, and assuming a blue colour when treated with the salt of iron,—possessing, in a word, all the known characters of this base; in such a case, the matter under consideration may be *affirmed* to be morphine.

Hydro-chlorate of Brucine.—On the 29th of March, 1826, eighteen grains of this salt, dissolved in a pint and a half of water, were introduced with intestine into a wide mouthed vessel, and exposed to the air. July 10th, the liquor, which from the beginning of April had exhaled a very fœtid odour, having been filtered, and precipitated by ammonia, yielded upon evaporation a product of a white colour, slightly inclining to yellow, which *reddened* strongly under the action of nitric acid. On the 12th of May, 1827, thirteen months and a half from the commencement of the experiment, the liquor restored the colour of litmus paper which had been reddened by an acid; it was turbid and brownish; assumed a dirty yellow colour when filtered, and furnished, when evaporated at a gentle heat, a solid yellowish product, which changed to a beautiful *red* colour on the addition of nitric acid; the portion thus reddened, passed to a *violet* when gently heated with a little proto-hydrochlorate of tin. This solid product was partly soluble in cold water; the filtered solution had a yellowish colour, and bitter taste; it was decomposed by ammonia, which precipitated *brucine* in a perfectly recognizable state.

Hydro-chlorate of Brucine greatly diluted with Water.—July 18th, 1826, six grains of the above salt, dissolved in a pint of water, were exposed with intestine, in the manner already mentioned. On the 13th May, 1827, ten months afterwards, the liquor was filtered, and deprived of its colour by heating it with animal charcoal, through which it was also passed repeatedly; evaporated to dryness at a very gentle heat, it yielded a slightly coloured product, which assumed a beautiful red when treated with nitric acid, and a violet colour, with the proto-hydrochlorate of tin.

Solid Hydro-chlorate of Brucine.—November 8th, 1826, twelve grains of the solid hydro-chlorate of brucine, meat, the white of an egg, and *soupe maigre*, were introduced into an intestine, and the whole enclosed in a thin deal box, and buried at a depth of two feet and a half. At the expiration of ten months it was disinterred, and the contents of the intestine repeatedly treated with boiling alcohol; the solutions were afterwards mixed together, evaporated to dryness, and the product agi-

tated with water, sharpened by acetic acid, in order that it might dissolve all the brucine, and not act upon the fatty matter; the solution, rendered colourless by charcoal and evaporated to dryness, afforded a bitter, yellowish residuum, which assumed a beautiful red colour by the action of nitric acid, and passed to a violet when subsequently treated with the proto-hydrochlorate of tin.

These experiments prove that the presence of brucine and of its hydro-chlorate, in the intestinal canal, may be demonstrated in a case of juridical exhumation, even several months after death. But here, as with acetate of morphine, the colours developed by nitric acid and the salt of tin, should be considered only as presumptive of poisoning, and in order to *affirm* that such has been the case, it will be necessary to separate brucine or its salt, and demonstrate its different characters.

Acetate of Strychnine.—May 11th, 1827, six grains of this salt, dissolved in a pint and a half of water, was exposed with intestine, as in the preceding case. On the 8th of August following, the mixture exhaled a fœtid odour; it was filtered, evaporated to dryness, and the product treated by alcohol, deprived of its colour by animal charcoal, and again evaporated, yielded a yellowish residuum, which acquired a *very beautiful red colour* on exposure to nitric acid, and possessed an insupportable *bitterness*, analogous to that of the salts of strychnine. It appears, therefore, that a salt of strychnine may be detected several months after its admixture with animal matters, even when the mixture has been exposed to the air.

Hydro-cyanic Acid.—The experiments of M. Lassaigne have proved, that minute quantities of this acid cannot be detected by chemical means, three days after death. Its disappearance in this case, is owing to its decomposition. (*Journal de Chimie Medicale*, Memoire de M. Lassaigne, tom. ii. p. 261.)

Opium.—A drachm of opium divided into pieces, a pint and a half of water, and several portions of intestine, were exposed together in a large mouthed vessel, May 16th, 1827. The sixth of August following, the mixture, which had become excessively fœtid, was filtered; in the matter remaining on the filter, fragments of a reddish brown colour were observed, which at first sight might have been taken for opium, but they possessed neither the odour nor texture of that article. The filtered liquor had a brownish colour, and strongly reddened litmus paper; it was treated with magnesia, alcohol, and animal charcoal, as in the process for separating morphine, and a solid, yellowish white, bitter product was obtained, to which nitric acid imparted a *beautiful red colour*; the trito-hydrochlorate of iron, however, changed it to a red, instead of a blue colour.

November 8th, 1826, a large intestine, containing ten grains of the aqueous extract of opium, the white of an egg, meat, and *soupe maigre*, was enclosed in a thin deal box, and buried at a depth of two feet and a half. On

the 18th July, 1827, nine months and ten days afterwards, it was disinterred, and the contents of the intestine treated repeatedly with warm distilled water, and subsequently with magnesia, alcohol, and animal charcoal, furnished a light residuum of a gray colour, slightly verging to yellow, which had a bitterish taste, and assumed a *clear orange red, but not very deep colour*, when treated with nitric acid; the trito-hydrochlorate of iron did not convert it to a blue.

It follows from these experiments, 1st, that the morphine which exists in opium, is not decomposed by contact with animal matter, any more than that which forms part of an acetate or other salt; 2d, that there is, nevertheless, more difficulty in demonstrating the presence of this base, in a case of exhumation of a dead body, into the alimentary canal of which opium has been introduced, than when the question relates simply to a salt of morphine; 3d, that in any case, we cannot pronounce *affirmatively* as to the fact of poisoning by opium, unless this article be recognized with its *physical and chemical properties*, which may be done even several days after death, or if this be impossible, unless morphine, possessing all the characters above indicated, have been extracted,—and even in this case, we are not absolutely to conclude that the poisoning has been effected by opium in substance, but either by opium, morphine, or a salt of morphine.

Cantharides.—An intestine, containing a drachm of pulverized cantharides, the white of an egg, and pieces of meat, was buried in a deal box, on the 8th of November, 1826. Disinterred on the 13th of August, 1827, the contents of the intestine were converted into a fatty matter, scattered over with a multitude of brilliant points, of a beautiful green colour, which were formed by the powdered cantharides. Treated by boiling water, the fat melted, and swam upon the surface in form of an oily layer, while the shining particles fell to the bottom; a sufficient quantity was thus collected, to place their nature beyond doubt.

We will not conclude this memoir, without resolving a question which might be asked. "The poisons which you have discovered in these different exhumations," it may be said, "were not brought into contact with our organs until after death; are we to infer, therefore, that they would be discovered in like manner, in researches upon the bodies of individuals who had been poisoned during life?" We shall reply in the affirmative, provided at the moment of death, there remain in the alimentary canal, a quantity of the poisonous substance, appreciable by chemical means. What does it signify whether the action of a poison upon our tissues, during life or after death, is or is not the same, or whether a portion of this poison has been absorbed, rejected from the stomach, or evacuated by stool, during the life of the individual? the capital point is to determine, whether a *quantity of poisonous matter*, which might have been discovered upon opening a dead body

24 hours after death, can be detected ten, fifteen, or twenty months after inhumation. Now our experiments leave no doubt on this subject, since these poisonous substances do not comport themselves otherwise in the alimentary canal of an interred body, than when mixed with articles of food, and enclosed in the stomach and intestines, as in the experiments narrated.

From the Glasgow Medical Journal.

CASE OF ASCITES, in which the *Abdomen was tapped through the Fundus of the Bladder, and an attempt made to establish a Fistulous Communication between the Bladder and Abdomen*. By ANDREW BUCHANAN, M. D., one of the Surgeons to the City Poor.

The mode of operating, and the subsequent treatment adopted in the following case, are I believe new; at least, I have not been able to find any traces of them in the surgical works I have looked into, nor in the course of the inquiries I have made among my professional friends. In the first volume of the Medical Communications (p. 361,) Mr. Henry Watson describes an operation for ascites, which he had practised three times successfully. Finding the vagina forming an external protrusion from the pressure of the fluid above, he introduced the trocar into the abdomen in that situation, preferring it to one higher up, as ensuring the more complete evacuation of the dropsical fluid. Mr. Watson's operation more nearly resembles the one here proposed, than any other I have read or heard of. My researches, however, upon this subject, have not been very minute, conceiving it of little moment. It is far more important to inquire, whether the operation described below be in any respect superior to the one commonly practised, and whether the plan of treatment subsequently adopted be calculated to supersede the necessity of repeating the operation; but it will be best, before entering upon these inquiries, to give the history of the case which suggested them.

Mrs. H. a middled-aged, married woman, in October 1826, was put upon the list of sick paupers, for ascites. By her account, the swelling of her belly had commenced about 18 months before, and after increasing to such a size as to induce her to consider herself pregnant, had gradually subsided, without the employment of any medicinal means. Twelve months ago, however, the swelling again appeared, and soon after became attended with anasarca of the legs.

When I first saw her, the abdomen was very much distended with fluid, the legs anasarcaous, and the urine scanty, with much debility and emaciation. Deriving no relief from the purgative and diuretic medicines prescribed for her, she was sent to the Infirmary. Medicinal treatment was there in like manner unavailing, and she was tapped before leaving the house.

On the 10th of November she again came under my charge; and as tapping was the only mode of affording her relief, that operation was performed upon her in the usual way, three times successively, each time about 28 pints of albuminous fluid being drawn off.

When the belly was emptied of fluid, and the hand applied to the left side, the spleen was felt very much enlarged. I was therefore led to think, that the liver was most probably diminished in size, and its substance converted into yellow tubercles, that being a combination I have frequently met with, and always found to occasion an incurable ascites.

The poor woman's case seemed thus utterly hopeless; for it was clear she could not long survive, if harassed by the continual re-accumulation of the water, and the repetition of the tapping. I was therefore induced to try an operation, which, if it succeeded according to my wish, would procure her more permanent relief. This operation consisted in tapping the belly through the upper and fore part of the urinary bladder. I conceived, that in the first place, the water might be drawn off in this way much more completely, than by the usual operation; for the puncture being made in the most depending situation, the whole fluid could be drawn off; whereas, when the puncture is made higher up, a considerable portion must remain lodged in the cavity of the pelvis. In the next place, I imagined, that the re-accumulation of the fluid might possibly be prevented, by the wound remaining in a fistulous state, being prevented from closing by the secreted serum constantly oozing downward into the bladder, to be thence discharged along with the urine. I had, however, well considered the possible danger and inconvenience that might be produced. I did not apprehend any difficulty in performing the operation; for it appeared to be, as I have since found it, both safe and simple. What I was most afraid of, was the risk of the urine getting into the cavity of the abdomen. On consideration, however, that risk did not appear so great as I at first supposed. If the contraction of the bladder were the sole expulsive power by which the urine is evacuated, that power, it is clear, would act with equal effect in forcing the urine through a hole in the fundus of the bladder, as through the orifice of the urethra. The principal agents, however, in expelling the urine, are the diaphragm and abdominal muscles, the action of which must tell chiefly on the lower part of the bladder, while it tends to shut up any opening in the fundus. This reasoning so far satisfied me, that I judged it right to attempt the operation, as the chance of doing good to the patient in this way had, in my mind, a decided preponderance. I therefore consulted my friend Mr. Watson on the subject, and had the satisfaction to find that his opinion coincided with my own. With this coincidence, I was the more gratified, as, from the extreme exhaustion of the patient, it was very probable, that whatever operation was performed, death might soon after ensue, and it might then have been supposed

that the mode of treatment had accelerated her fate.

Next day (Thursday, January 4th, 1827,) I proceeded to perform the operation, assisted by Mr. Alexander. The patient lay upon her back across the bed, her feet resting on a stool placed at the bedside. The instrument employed was the curved trocar, for puncturing the bladder in case of retention of urine. The canula was first introduced along the urethra, and the point of it carried towards the upper and fore part of the bladder, pushing it as far up as possible, so as to put the coats of the bladder over its orifice upon the stretch. The stilette was then passed along the canula, and made to pierce the bladder, which it did with such ease, that no sensible resistance was experienced. On withdrawing the stilette, the water flowed in a full stream, till nearly as much had been drawn off as on any former occasion. Some interruption then took place from time to time, seemingly from the fluid subsiding below the level of the orifice of the canula, or from portions of intestine covering it up. By drawing the instrument downwards, however, and shifting it about, the water continued to flow freely, and upon the whole, a greater quantity was drawn off than had ever been done before, although a shorter period intervened between this and the last operation, than on any former occasion. Some pain and bleeding were produced on passing the canula along the urethra, the orifice of which was obstructed by large carunculae; pain was also excited by the stretching the bladder, and the piercing of it.

Felt much relieved by the operation. Passed the night without pain, but was restless and slept little. Vomited this morning, and still feels squeamish. No pain of abdomen on pressure. Pulse, as for some days past, about 100. Tongue also as before, pretty natural after drinking, but soon becoming dry in the middle. Several loose stools. Urine passed freely, and in much larger quantities than before the operation, containing some clots of blood, most probably from urethra. Ordered some soup.

January 6th.—Sickness diminished, and no pain any where. Moans, however, as if uneasy. Belly tympanitic. Soup, of which she had taken about four times the quantity ordered, had refreshed her, but produced purging, probably also the tympany just mentioned. Urine passed in a forcible stream, and, on introducing the catheter immediately after, bladder quite empty. Castor oil and a little solid food.

7th.—Vomiting returned; only of ingesta. No pain. Tympany diminished. Pulse 108, softer than yesterday. Tongue moist. Urine comes freely. Castor oil not taken. Relished food. Common bolus.

8th.—Pulse 104, soft. No vomiting since 10 last night. Less urine. Still tympany.

9th.—Pulse 100, soft. Tongue moist and pretty clean. Vomiting occasionally; also pain of stomach, relieved by some pepper taken in warm milk. Urine more scanty.

10th.—Pulse 108. Tongue clean and moist. Much debility. Belly fluctuates. Urine very scanty.

14th.—Pulse 84. Tongue moist. Abdomen larger, with distinct fluctuation. Urine scanty.

20th.—On the evening of the 17th, the swelling and tension of the abdomen had increased so much, that, at her own urgent request, the water was again drawn off. The operation was performed exactly in the same way as before. She seemed to suffer very little pain, except from passing the instrument along the urethra. When about 24 pints had come away, the canula was withdrawn, although there was still sensible fluctuation in the abdomen. This was done with the view of facilitating the formation of a fistulous opening into the bladder. Next day, when I called to see her, she was out of bed, sitting by the fire-side, which she had not been able to do since the previous operation. Urine passed with ease, but in small quantity, much less than the day after the first operation. On examining it, and comparing it with some voided before the tapping, it had exactly the same appearance. It was high coloured, and deposited on cooling a copious pink sediment, *resoluble* on applying heat. No trace of albumen could be detected in it, either by heat, or nitric acid. When visited to-day, had gone out.

February 8th.—On the 23d ult., the swelling had increased so much, that at her own request, the tapping was again resorted to. As, from the result of the two trials already made, there did not seem much chance of a fistulous opening being established by the mere oozing of the dropsical fluid, I began to think of some other means of ensuring the accomplishment of the object. Mr. Watson suggested the introduction of an elastic catheter, to which, however, I preferred a catgut bougie of the smallest size, which would not prevent the transmission of the urine along the urethra. When the water had been nearly drawn off, I intended to introduce the bougie through the canula, and then withdrawing the canula, to leave the bougie in its place, to be there fastened, and kept for some days, occasionally moving it a little.

In performing the operation on the two previous occasions, the principal pain was produced by the sharp edge of the canula irritating the sides of the urethra. To prevent this, I passed along the canula, before introducing it, a white bougie, the end of which exactly fitted the upper orifice of the canula, while the body was shaven down so as to admit of its being passed. By this means the canula glided along the urethra without producing any pain. The bougie being then withdrawn the stilette was introduced, and the fundus of the bladder pierced as before. The water did not come away so freely as formerly; and it was likewise tinged with blood. These appearances induced me to think, that the end of the canula had not passed fairly into the abdominal cavity, but was overlapped

in part by a fold of the bladder. I have seen the very same appearances produced by the end of the canula not passing fairly beyond the parietes of the abdomen. When about four pints had been drawn off, we were obliged to desist by a diarrhoea, to which the patient had been subject for some days previous.

This accident prevented the trial of the proposed plan of introducing the bougie, and the patient conceiving an aversion to the new mode of operating, the water was drawn off in the usual way, on Sunday the 27th ult. Fully more than the usual quantity came away.

February 9th.—Yesterday the tension of the belly again producing the most distressing suffering, the patient wished the water drawn off; and though her debility was now so extreme, that I entertained no hope of her long surviving, yet as I had already more than once been deceived in my prognostic regarding her, I judged it right, to give her the small chance, which the use of the bougie held out, of obviating the necessity of repeating the tapping. The patient was easily induced to consent to the trial, her only objection to the *bladder operation*, arising from the exposure of her person, while she preferred it as giving her less pain than the other operation, the cut required for performing which, on the 27th ult., was not yet healed.

The canula being introduced armed with the bougie as on the last occasion, the water flowed away in a full stream till nearly the usual quantity was obtained. Then, while there was still distinct fluctuation in the belly, and the stream of fluid uninterrupted, the catgut bougie was passed along the canula, till it had gone a couple of inches beyond the upper orifice into the abdomen, as was ascertained by two marks on the bougie, one the length of the canula from its upper extremity, and the other two inches farther off. The bougie being now held firmly, the canula was drawn over it, as also over another larger one, applied to the end of the small one, for the purpose of keeping it steady in its place, till the canula was completely disengaged. Tapes were fastened to the bougie in the same way as for keeping a catheter in the bladder. The debility of the patient was so great, that I was afraid more than once, she would not outlive the operation.

During the night, about a quart of fluid came away. To-day about 3 P. M., some water was obtained for examination, but it gave no trace of albumen. The patient had, however, previously been out of bed in a room adjoining her sleeping room, so that the end of the bougie might have been displaced. Says she feels easy. Pulse quick and feeble.

February 24th.—When visited on the 10th inst. the day after last report had gone out, and that day, as she afterwards stated, the bougie came away. On the 20th, the water having re-accumulated so as to produce more swelling than on any former occasion, the fundus of the bladder was again penetrated, and 48 pints drawn off. The stream not flowing freely at first, I withdrew the canula and made a second

puncture. The increasing debility and wretched accommodation of the patient, prevented any farther attempt at the establishment of a fistulous opening. Next day felt much relieved, but very feeble. To-day, when visited, was sitting by the fire, in a room adjoining that where she slept. Swelling beginning to return, but not yet productive of any uneasiness.

March 3d.—On the 27th ult., I was very much surprised at receiving a visit from my patient. She came to inform me of the re-accumulation of the water, and I promised to draw it off for her next day. Her extreme emaciation, hollow eyes, and ghastly appearance, contrasted with the extraordinary prominence of the abdomen, excited the attention of the passengers, as she walked along, with no other support than that of a stick. As this visit showed a degree of vigour, I had no idea she possessed, I resolved to attempt once more the establishment of a fistulous opening in the bladder. The mode of doing it, I had been thinking of some time before, but not expecting so soon to have an opportunity of trying it, I had only adverted to the general principles of the operation, without attending sufficiently to the mode of reducing them to practice. I proposed, before the fluid in the abdomen was fully evacuated, to introduce through the canula a small inflated bladder of a longish shape. This bladder, being introduced into the canula according to its long diameter, would admit, by a little compression, of being forced along, and, when again free to expand in the abdominal cavity, would float in the dropsical fluid, while its size and buoyancy together, would prevent it from descending through the opening made by the trocar. Attached to this floating bladder a silk thread was to pass into the urinary bladder, and thence along the urethra, to be fastened externally. The essential part of the apparatus, however, was to consist of three or four thick worsted threads, having the same attachment, at the upper end, as the silk one, and passing along with it into the bladder, but no farther, their lower ends being fastened to the portion of the silk thread within the bladder. Along these worsted threads, I presumed, the dropsical fluid would ooze down into the bladder, partly by gravity, and partly by capillary attraction, in the same way as in the well known experiment, by which a tumbler of water may be emptied by similar threads passing over its side. When this apparatus had remained a sufficient length of time to prevent the wound of the bladder from closing, or if, from the occurrence of irritation, or any other cause, it were thought advisable to withdraw it, that could at once be effected by re-introducing the canula, and guiding it upward along the silk thread, till it came to the small bladder, which being punctured with a fine stilette, the whole could be withdrawn. If, on the contrary, no irritation supervened, and it were not judged prudent to withdraw the apparatus, it might perhaps be possible to dispense altogether with the silk thread, passing along the urethra, that

thread terminating like the worsted ones in the urinary bladder, and being there attached, exactly as at the other end, to a small inflated bladder, which would prevent the threads from being drawn up into the abdomen, and probably also, by floating in the urine, render them less apt to be crusted with saline matter.

A small bladder, strong enough to admit of the necessary compression being the only thing wanted, I first thought of the swim-bladder of a fish. Next day I procured some fish, but found the swim-bladder, and also the bowels, far too weak for the purpose. I delayed another day, that I might try the intestines of some birds, but these being also insufficient, I thought I could no longer protract the suffering of the patient by deferring the operation. I accordingly punctured the bladder upon the 1st inst., drawing off about the usual quantity of fluid.

March 8th.—When visited, the day after the operation of the 1st inst., she had gone out, and I heard no intelligence of her till the 5th, when her husband came to inform me of her being very ill. That afternoon I visited her and found her speechless, and exhibiting no mark of sensibility, except moaning when moved from one position to another. By the account of her husband, she had been nearly in the same state since the 3d inst. She got some whisky toddy, and survived till yesterday morning.

Dissection.—There was a good deal of swelling of the belly, arising partly from a tympanitic state of the upper part of the colon, and partly from the re-accumulation of the dropsical fluid, with which the pelvis, and about a third of the abdomen, were filled. The liver was shrunk into a very small compass in the right hypochondrium. It was nodulated, and uneven on the surface, and completely tuberculated throughout. Spleen considerably enlarged. Kidneys healthy. The peritonæum had everywhere its usual glistening aspect, totally free from any appearance of inflammation, as might indeed have been inferred from the copious secretion of serum, which could not have gone on, had an attack of inflammation supervened on the last tapping.

The state of the bladder was the point I was most anxious to ascertain. On the fundus of it five marks from puncturing could be distinctly recognized. They were all between the uterus and the symphysis pubis, three of them within a few lines of each other, and the other two about an inch distant. The last two punctures could be most easily recognized. The most recent, made six days before death, had left a spot of a deep red, or rather grayish black appearance, rather larger than the orifice of the canula. It had very much the appearance of a spot produced on the skin by blood extravasated under the cuticle, being distinctly circumscribed, and the surrounding peritonæum perfectly healthy, and free of inflammation. The muscular coat had adhered, the opening being perfectly closed. The wound from the second last puncture, made fifteen days before death, had exactly the tri-

cuspidate shape, and the size of a leech bite, three minute ulcerated lines meeting in the centre, and the ulceration not extending deeper than the peritonæal coat. Nothing could be more exact than the resemblance of this wound to a leech bite, which had been prevented from healing by the application of a poultice, only there was neither surrounding redness nor swelling. In a third wound, the tricuspidate shape could no longer be recognized, but a very small superficial ulcer was still distinctly visible.* In a fourth wound, most probably made 27 days before death, I was at a loss to say whether any ulceration remained, but I was inclined to think there was still a little. The fifth puncture was completely cicatrized, and observed only when minutely inspected. As the operation was performed six times, and at one of them the bladder was twice punctured, the situation of two of the wounds remained undiscovered. I wished to bring away the bladder entire, that I might examine it more minutely at leisure, but this the poor woman's husband would by no means permit; and while endeavouring to persuade him to allow it, I regret to say, that I forgot, while it was yet in my power, to examine the state of the mucous coat of the bladder.

The history of the preceding case has been so minute, and the discussion of the most important parts of it so blended with the historical details, that there is little room left for general observations. I shall, however, offer a few remarks, first, on the operation of tapping through the fundus of the bladder; and, second, on the establishment of a fistulous communication between the bladder and abdomen.

The operation of tapping the abdomen through the fundus of the bladder presents no difficulty in the performance, to any one who can introduce the catheter, and knows the relations of the bladder to the other viscera of the abdomen. For operating on the female, perhaps no better instrument could be devised than the armed canula, and stilette employed in the preceding case. In the male, the operation would be more difficult, from the length and curvature of the urethra, and the want of a proper instrument; it would, however, I think, require little ingenuity to adapt such an apparatus to a male catheter, as would perfectly answer the object in view. Of hæmorrhage, there seems to be less chance in this operation than in the ordinary one; and as for wounding the viscera of the abdomen, I should think it scarcely possible, with an ordinary degree of care; for not only are the intestines kept out of the way by the position of the patient, and the accumulation of fluid in front of them, but if the canula be carried fairly up to the fundus of the bladder, the stilette will pierce the membrane stretched over the mouth

of the canula, with as much ease as a bit of paper, and it may therefore be introduced with so much caution, that a noose of intestine accidentally above it (if not actually adherent) would be pushed aside without being wounded. The pain produced by puncturing the bladder is very inconsiderable, not greater, I should think, than that occasioned by opening a vein of the arm. The dropsical fluid is more completely evacuated than by the ordinary operation. The wound inflicted heals readily, and does not occasion any inconvenience while it remains open. The patient in the preceding case was generally out of bed, and felt no pain the day after the tapping; whereas, when the ordinary operation was practised, the wound was yet painful and unclosed, when it became necessary to repeat the operation. Lastly, if, as is thought by many, the inflammation of the peritonæum after tapping is in some degree to be ascribed to the access of the atmospheric air to the abdominal cavity, that cause of irritation is avoided by operating through the bladder. To counterbalance these advantages must be placed the risk of introducing urine into the abdominal cavity; the amount of that risk, however, can only be determined by experience; and so far as the experience derived from the preceding case entitles us to judge, it does not constitute a serious ground of apprehension.

As for establishing a fistulous opening between the bladder and abdomen, by some such apparatus as that described in the report of March 3d, I think the objects to be obtained of sufficient importance to warrant the experiment. Many objections to it have indeed occurred to myself, or been mentioned to me by my professional friends, as the insufficiency of the apparatus, owing to the collapse of the bladder from the pressure of the superincumbent fluid—the danger of the influx of urine into the abdomen—the irritation of a foreign body in the bladder and abdomen—the formation of a hernia, and the furnishing a nucleus for urinary concretions. Objections like the last, founded on circumstances which can only become formidable in the course of years, can scarcely be urged against a plan, intended to relieve a disease, which threatens to be speedily fatal. What weight should be attached to the other objections, it would, as appears to me, be idle to examine, since the most ingenious reasoning, derived from general principles, can never produce conviction in questions which experiment alone can decide. If the operation of M. Dupuytren for the cure of fistula lachrymalis had been first announced to the world, not under the sanction of experience, but in a speculative form, there can be little doubt, that critics would have contended in denouncing it as visionary and impracticable. I cannot but think, therefore, that to form a decided opinion of the practicability of the plan here proposed from abstract arguments alone, would be little consistent with the spirit of the inductive philosophy. How the bladder will comport itself in the circumstances here supposed, and how the adjacent

* This wound I was disposed to think of the same date as the last, two punctures having been made at the operation of the 20th ult.

parts will be affected, we do not know, because we have not tried. Whatever conjectures we may form as to the probable result of the experiment, it is by trying it alone that a certain judgment can be obtained. I should be glad, therefore, that this paper were to fall into the hands of medical gentlemen having the charge of hospitals, or other public institutions for the cure of the sick, that if any of them were disposed to take the same views as myself of the mode of treatment proposed above, a trial might be made of its efficacy.

From the Edinburgh Medical and Surgical Journal.

CASE OF MONOMANIA, CAUSED APPARENTLY BY CIRCUMSCRIBED CHRONIC MENINGITIS, WITH REMARKS. By DAVID SCOTT, M. D.

John Anderson, M. D., aged 41, of fair complexion, sanguine temperament, and endowed by nature with a healthy constitution, was, after the general peace in 1815, placed on the half pay of the navy, in which for many years he maintained the character of an active and zealous member. He came to reside in Cupar, his native town, where, from the extent and variety of his information, the sociableness of his habits, and urbanity of his manners, his company and conversation were generally courted. He contemplated settling somewhere, but his designs in this respect were always frustrated; and from a repetition of disappointments, conjoined with domestic causes, he became liable to occasional fits of despondency. About the year 1820 his intimate friends began to remark a peculiarity of manner, and singularity of ideas; but these were only occasional, and during times of temporary excitement. Addicted to study from early youth, and distinguished for his acquirements in learning, he continued to read much both of English and French authors; and in the course of his reading he met with some articles that treated of animal magnetism, which struck his attention, and made a great impression on his mind. Pondering long on this subject, and beguiled by the subtle and speculative reasoning of these authors, he began first to admit the possibility of animal magnetism, and then imagined that he himself was subject to its influence. Soon afterwards, from an occasional incoherence of action, and strangeness of exclamation, it became apparent to his friends that this opinion was assuming an ascendancy over his mind. They endeavoured of course to scout it as nonsense, and to rail at him for allowing so vague a chimera to disturb his thoughts; but still the idea gained ground, and in a few years took so firm a hold of his imagination, that what was formerly merely admitted as possible, now became matter of serious truth and cause of apprehension. He no longer attempted to conceal his belief, but avowed it openly, and even accused some of his best friends and acquaintances of being accessory to keeping him under its influence,

and holding him in a continual state of alarm. His nights in consequence became greatly disturbed, and he seldom obtained any satisfactory rest. If he did sleep, he was tormented by oppressive dreams and other strange phantasms. His notion of animal magnetism was, that certain individuals, who had an antipathy to him, could wield at will an influence over him of so malignant a nature as to deprive him of every kind of enjoyment, and keep him in a continual state of discomfort and anxiety. He invested these *invisibles*, as he called them, with vast power. No place was proof against their malignity, nor could distance restrain it. He went to Paris in the year 1822, with the view of escaping from it, but he found its influence there as great as at home. He frequently during the night could hear his enemies planning schemes for his annoyance. In his imagination they had recourse to every kind of torment which the most wicked and inquisitorial minds could invent, and were inexorable and persevering in their attacks. In the night-time, for example, they would rest on his breast with the weight of a millstone, deprive him of sleep, disturb his digestion, lock up his bowels, &c. &c.; at other times they would stimulate the bladder and rectum so powerfully and so immediately, that he had not time to undress himself; and on some occasions they would take such unwarrantable liberties with him, that he was compelled, in self-defence, to roar out loudly, by which he thought he obtained a relaxation of annoyance. Several times he made application to the local authorities to control their malignity, and even took bond from some of his acquaintances that they should cease to disturb him. On all other subjects, saving animal magnetism, his judgment was sound, and indeed in reasoning he evinced much acuteness; a stranger, in short, when the peculiar subject was not agitated, could not detect any thing unusual about him. Latterly, however, he complained of an impairment in memory, and that of a peculiar kind. He could not recollect the words he was accustomed to express his ideas in; he would therefore stop during speech and labor for them; he seemed to perceive them as it were in his mind, but could not get his recollection to lay hold on them. He continued in this state for several years, without apparently suffering in health, or losing much flesh.

About a year before his death, he had several attacks of pneumonia of the left side. On these occasions, though seriously ill, there was considerable difficulty in getting him to submit to medical treatment. Having a great aversion to medicine, he considered it in his case as useless and inert, and by no means calculated to remove the cause. At these times the only chance of success with him was, to admit his premises, but to draw very opposite conclusions. With this view we acceded to his notions about animal magnetism, and held, that, so long as its influence was applied to the body generally, its effects were innocuous; but so soon as so powerful a stimulant

was for any length of time directed upon any single organ, the result could not be otherwise than hurtful, and would terminate in inflammation. In this manner, though he pertinaciously retained his opinion as to its cause, he would allow us to combat its supposed effects, and submitted to be bled, blistered, &c. &c.

For several months before he died he had a short dry cough, accompanied with a sibilous sonorous respiration; he complained also of a severe pain in the back, with an oppression and tightness across the chest, which he compared to an iron girdle. He was greatly dispirited, and confined himself very much to the house. His pulse, (unless when he was labouring under one of his inflammatory attacks, when it was hard and quick,) felt always soft, equable, deep-seated, and feeble, but never intermittent. The sound of the heart was hardly perceptible, but its contractions appeared regular. The lower portion of the left breast and back sounded fleshy on percussion, and the respiration in these places was not perceptible by the stethoscope; but higher up, and opposite the bifurcation of the trachea, it was sibilant and sonorous. It was considered that, in addition to monomania, he laboured under chronic bronchitis, with some hepatization of the lower portion of the left lung; and from the treatment applicable to these he derived relief, though the noisy respiration never left him.

On the day of his death he had invited a few friends to dine with him. They had assembled, and all was ready; when suddenly their worthy host was seized with coughing, hawking, and spitting of blood, which increased so rapidly, and came forth in such vast quantity, that in a few minutes, being unable to expel it, he died suffocated.

Dissection.—The extreme suddenness and singularity of his death created a desire on the part of his acquaintances, that the cause of it should be ascertained; I therefore examined the body in company with Drs. Grace and Spens. The body was not emaciated, there being a considerable quantity of fat in the cellular membrane. In the cavity of the abdomen we could discover nothing unusual, unless that the liver was considered larger than natural, but not diseased in structure. In the chest, after the sternum and cartilages of the ribs were removed, we found about a pint of fluid blood in each of its cavities. There was interlobular adhesion of the left lung, but none between the *pleura costalis* and *pulmonalis*. Its inferior lobe felt hepatized and completely gorged with blood, the bronchii and air cells being filled with it. After tying the blood-vessels in the neighbourhood of the heart, we endeavoured to remove the trachea, bronchii, and both lungs from the cavity of the chest, in order to give us more room to trace the source of blood. In doing this we discovered a large aneurism of the descending thoracic aorta. It had its seat in that vessel immediately after it gives off the left subclavian, contained about a pound of fibrinous matter in concentric layers, occupied the

whole cylinder of the tube, and pressed strongly on the roots of the bronchii, and on the vertebræ of the back, so that a considerable portion of the vertebræ was eroded and absorbed. On minutely examining its connexions with the surrounding parts, we found a large opening communicating with the left bronchial branch of the trachea, the rupture into which sufficiently explained the suddenness of death, and the presence of blood in that tube and other parts of the lungs. The right lung, though filled with blood, was otherwise perfectly healthy.

We opened the cavity of the cranium, and observed with some attention the condition of the brain. The veins on its surface, and particularly where they entered the longitudinal sinus, were very turgid with blood, but this we considered as accidental, and arising from the suffocating manner of death. There was an inflammatory deposit, apparently of old standing, under the arachnoid coat, with thickening of the membrane itself, and adhesion to the parts beneath for about the space of an inch and a half in length, and one in breadth, on each side of the longitudinal sinus, midway between the *crista galli* and the level of the commencement of the lateral sinuses. The cortical substance of the brain under this spot was not changed in appearance, or altered in structure. We could observe no other morbid alteration in any part of the membranes or of the brain proper, either as to softening or hardening or other morbid change. Nor was there any watery deposit in the lateral ventricles, or any thickening of their lining membrane. The cerebellum, *tuber annulare*, *medulla oblongata*, and all the nerves issuing from the brain, appeared to be perfectly natural.

Remarks.—We have seen from the dissection that our patient laboured under not only an illusory complaint, but also under a serious organic disease, which escaped detection during life. This is one of the misfortunes to which people in his condition are liable. They feel a variety of imaginary sensations, sufficiently intense for the time, but, from their evanescence and sudden change of character, they are little regarded either by the patient himself or his attendants. Occasionally, however, these deceptive symptoms are mixed with those arising from actual disease; and it is from a combination of this kind that arises the difficulty of separating the true from the false; but whenever they begin to assume a permanency of character and situation they ought to be considered as real, and demanding attentive consideration. For example, the fixed burning pain which my patient long complained of in his back, and a sharp lancinating pain which he referred to the left ventricle of the heart, were both evidently owing to disease; the first caused by the erosion of the vertebræ of the back from the pressure of the aneurism, and the latter to inflammation of the lower portion of the left lung. Yet it is remarkable that both these pains frequently subsided and disappeared for

days and weeks, and were succeeded by others of an anomalous character. Here, therefore, the distinction between the true and false symptoms is not so easily drawn; and when the patient labours under the misapprehension of the nature of his complaint, and attributes all his feelings to some uncommon cause, as in the present instance, it is exceedingly difficult to arrive at a just diagnosis. The sibilous and sonorous respiration was evidently produced by the pressure of the aneurism on the roots of the bronchii, particularly the left, diminishing the caliber of the tube, and thereby obstructing the ingress and egress of air to and from the lungs. There was no intermission of the pulse, because the aneurism was seated beyond the giving off of the subclavian arteries, neither had he ever any unusual pulsation in the chest, or any labouring or palpitation of the heart. The latter organ was found healthy, which accounts for its natural action; and the only indication of the diseased aorta was an indescribable anxiety and perpetual uneasiness, which he was unable to refer to any particular place, which constantly tormented him, and which he as constantly referred to some invisible agency. I do not think, however, that the disease in the chest had any thing to do with the aberration of mind, although it might contribute to exasperate it.

The hallucination in reference to animal magnetism is a subject much more difficult to unfold, as its investigation embraces the connexion subsisting between mind and matter. That this hallucination was caused by an irritation, or chronic inflammation of a fixed part of the surface of the brain, appears to me exceedingly probable; because, in the first place, an inflammatory deposit under the covering membranes, with adhesion to the substance beneath, was found on dissection affecting a small space on the surface of the brain; and, secondly, because the mind was unhinged only on one particular point. On all other subjects his judgment was entire, and he could discourse as rationally as any other man; and in like manner on dissection we found all other parts of the brain perfectly healthy. There would appear to be something here like a chain of cause and effect. I shall not go so far as to maintain with the phrenologists, that the mental powers of man and other animals depend upon the size and development of certain parts of the brain, or that the peculiarities of individuals are always owing to a peculiar conformation of that organ,—merely because, from not having yet directed my attention to that subject, I am unable to form any precise opinion on the matter. But from what I have heard, and the notice taken of it by some periodicals, I think it bears in the outline a great degree of plausibility.

It is exceedingly natural to think, that the organ through which alone are manifested the thinking powers or mind of an individual, must be influenced, or entirely subverted, according as the brain is either simply irritated, or altered and destroyed in structure; because the like thing happens in other organs of the

body. Farther, it is probable that all the nerves of sense have a distinct termination in the brain, or that certain portions of it are peculiarly destined for the development of these senses. Hence the sense of hearing may be affected without injuring the sense of sight. And if this condition hold with regard to the external senses, it is extremely reasonable to think that the like circumstance obtains with the internal, and that each has a located portion of the brain through which it is peculiarly manifested; and by consequence, if any of these parts be injured or destroyed, the properties of the mind which depend on them will in like manner be injured or destroyed. The difficulty of the science would appear to consist entirely in discovering these parts, and allotting to each its proper function. To accomplish all this would require an immensity of observation continued through many years, nay ages; but notwithstanding these acknowledged difficulties, if the proposition be founded in truth, they ought to deter no one, desirous of tracing nature to her utmost recesses, from attempting to surmount them; for if this object could once be attained, there is no doubt that it would greatly contribute to simplify the treatment of diseases of the brain. The mind itself is totally unsusceptible of disease; for who can suppose that an incorporeal substance is liable to diseased action. It is an obstruction or obscurity in the organ alone that causes an imperfection or obscurity in the mind, in the same way that dim spectacles will always convey images to the optic nerve in a dim manner. If they happen to be stained, then all images will come to the mind with the same hue; and should they be opaque, none will be transmitted. An opacity of the lens of the eye is no proof that the optic nerves are incapacitated from receiving the impression of images. If the opacity be removed by nature or by an operation, the individual will see as before. In like manner, if the obstruction or incapacity of the affected part of the brain be removed, the mind will be as entire as ever. These observations would go to prove that mania, monomania, hypochondriasis, &c. are corporeal diseases, and that we should first endeavour to remove the physical incapacity of the injured organs, and afterwards confirm the cure by regimen and moral restraint.

But while we give our cordial assent to the outline of the system advocated by the phrenologists, yet we are by no means certain, that the localities they have pitched on as the organs chiefly concerned in the evolution of the mind, if we may so speak, are correct; because, in the first place, it is very probable that it may require the co-operation of two or more organs to express even the most simple idea; and, secondly, I really do think it would require an immensity of observation, and comparison of heads of every size and form, besides dissections of cases such as the present, before even the function of a single portion of the brain could be permanently established, and far more before the whole of the organs

composing the brain could be reduced into any thing like system.

The phrenologists have greatly injured themselves by their pertinacity in adhering to system; for, instead of making observations, and recording facts to serve as materials for some future building of the science, they have at once assumed that their work is so perfect as to be practically useful. Now this rapidity of perfection has never obtained with any of the sciences depending on observation;—a remark, of which the sciences of medicine afford many a melancholy example.

From the London Medical Gazette.

SUCCESSFUL EXTIRPATION OF AN HYDATID SITUATED IN THE PELVIS, and preventing the Passage of the Urine and Fæces.

Mrs. B., 38 years of age, of a lymphatic temperament, suffered, about eight years ago, from an unusually long and difficult labour. The accoucheur found that this difficulty arose from a tumour in the pelvis, on the left side of the vagina, and did not conceal from the patient the obstacles that might render delivery in future even more difficult. This tumour increased slowly, but without occasioning any inconvenience for four or five years. Within the last three years, however, Mad. B. began to experience some inconvenience in going to stool and passing her urine. These impediments continued to increase. At length the ischury became complete, and passing the fæces almost impossible. The husband of the patient had learned to draw off the patient's water, which he did four or five times a day. Such was the patient's condition when, in the middle of March, she presented herself at La Charité. Upon examination, a resisting tumour was discovered on the left side of the vagina, extending from the margin of the pelvis to the basis of the labium. It pushed the vagina towards the right side, and appeared to be immoveable; but the parietes of the vagina readily glided over it. M. Roux thought that he perceived a fluctuation at one point; but it was so obscure that it did not at all clear up the diagnosis. It was thought necessary to attempt its extirpation; but its situation, and the vicinity of the hypogastric arteries and their principal ramifications, rendered any operation serious: the melancholy condition of the patient alone appeared to warrant the attempt. MM. Boyer and Roux thought it practicable, though dangerous, and the last named gentleman performed it on the 20th March, without exactly knowing, when he commenced it, in what manner it was to be terminated. He had formed the project of exposing the tumour by means of an incision in the form of the letter T, one portion of which would divide the left side of the vagina longitudinally, and the other the labium lenthways. The two flaps of this incision might be dissected, and the tumour laid bare.

The patient being laid upon a table, the but-

tocks upon the edge, the lower limbs were supported by two assistants. The surgeon, standing in front, introduced the index finger of the left hand deep into the vagina; he then passed the blade of a straight bistoury along the finger, and turning the cutting edge towards the left side of the cavity, he made an incision from above downwards; immediately a diaphanous liquor flowed out, of a straw colour. The finger penetrated through this incision into a spacious cavity, to the sides of which membranous fragments appeared to be adhering. Some portions of these were removed with common dressing forceps, of a pearly white colour, and of a gelatinous consistence: these were evidently the remnants of hydatids. A pair of polypus forceps was then introduced, and a large body of the same nature extracted. The extensive cavity thus resulting from the extirpation of the hydatid was filled with charpie, to which a string was attached.

The day of the operation, the patient did not experience the least pain, but the urine passed away involuntarily. Towards evening fever came on, and there was no sleep during the night.

On the 25th, although the urine still passed away involuntarily, and there had been no faecal evacuations, the patient was in other respects better. Some of the lint was removed from the cavity, covered with pus; the rest, still adherent, was suffered to remain. The next day the bowels were open; there was no pain, and every thing was going on well. In the evening of the same day an abundant hæmorrhage ensued from the wound, in consequence of the pipe of an injecting syringe having been incautiously introduced into it; the bleeding was stopped by plugging up the wound, but not until syncope had taken place.

On the 31st, there was no longer any doubt of the patient's doing well—the suppuration was becoming less from day to day.

This operation gave occasion to M. Roux to make some remarks upon hydatids. He observed, that these cysts were frequently developed in the internal organs, but that, as they did not often become surgical diseases, they scarcely ever found a place even in the most complete surgical works; nevertheless, he had often met with cases in which the nature of the tumour had only been ascertained after the operation. He mentioned two other instances.

One day, he was called upon to give his assistance to a woman who had a tumour at the navel, which was universally thought to be a rupture. Ulceration had spontaneously taken place upon its surface, exposing a membranous sac, which was believed to be that of a hernia, but it was not reducible. The woman was suffering from some of the symptoms of strangulated hernia. M. Roux thought it necessary to make some incisions, for the purpose of relieving the supposed strangulation; but, after having done so, he discovered that the tumour was merely an hydatid.

On another occasion, he was consulted by

a female who was affected with a large tumour, of an irregular surface, in the left breast. This tumour had existed about seven years, and had increased by almost insensible degrees. There never had been any lancinating pains; the glands in the axilla were sound, but there was a sense of weight and of tension in the surrounding parts. M. Roux removed the breast, which he found to be entirely possessed by a series of hydatids.

Last year, a man was admitted into La Charité, who had, at the posterior part of the shoulder, a fluctuating tumour, which was believed to be a chronic encysted abscess. It was opened, and a transparent straw-coloured fluid escaped. Severe symptoms followed this slight operation, and the man died. On opening the body, an enormous hydatid was discovered, situated partly in the infra scapular fossa, and partly in the infra spinous fossa.

When M. Roux was surgeon to the Hospital Beaujon, nearly 18 years ago, a young girl was admitted into the physician's ward, on account of a large tumour which was felt deeply situated in the right hypochondrium, under the edges of the ribs. This tumour was evidently situated in the liver, but its nature was not clear. The girl was in excellent health, and it did not prevent her from taking exercise and pursuing her occupations. One day, in consequence of some exertion, she suddenly felt an acute pain; the tumour disappeared, but the lower part of the abdomen became tumefied, and fluctuation was very perceptible at that point. An incision was made at the lower part of the linea alba, which gave vent to a transparent straw-coloured fluid, in which a great number of hydatids were floating. The patient died soon afterwards. On opening the body, a great number of hydatids were found in the cavity of the abdomen, and in the liver there was an enormous cyst, which had been ruptured, and the effusion from which into the abdominal cavity had caused the patient's death.

From the Journal de Physiologie, &c.

LA VUE PEUT-ELLE ETRE CONSERVEE MALGRE LA DESTRUCTION DES NERFS OPTIQUES? Par M. MAGENDIE, Membre de l'Institut.

We have already related in this journal, experiments which prove incontestably, that the fifth pair of nerves is the principal organ of the general sensibility of the head; that it is very probably the agent of smell; that it is certainly that of taste; and, what is more extraordinary, that it maintains in dependence the senses of sight and hearing, in imparting to them through some unknown influence, their peculiar sensibility. Thus, when the fifth pair is divided on both sides, sight is destroyed, notwithstanding the iris is still sensible when exposed to the direct rays of the sun. But although analogy might lead to such a supposition, we had never ventured to suspect that the fifth pair could supply, even

temporarily, the action of the nervous apparatus of the eye, that is to say, the optic nerve. The following pathological fact will justify us in regarding the thing, if not proved, at least as possible; and at all events, will remove from the conjecture, any appearance which it might have of a paradoxical character.

We shall proceed then, to relate this interesting case, communicated by Dr. Sanson, surgeon of the Hôtel Dieu of Paris, as drawn up by M. Corbin, élève interne, who has confirmed to me, viva voce, all the details therein contained. It is greatly to be lamented, that this piece of pathological anatomy was not carefully preserved; every one might then have assured himself of a fact, thus far, I believe, unique in the history of the science.

Hôtel Dieu de Paris, Salle Saint Bernard.

Bardon, a writer, unmarried, aged thirty-six years, an inhabitant of Orleans, entered the hospital September 8th, 1827, and died on the 21st November.

Amaurosis—cyst, in part osseous, situated posteriorly to the commissure of the optic nerves.

At the period of his admission, the pupils were greatly dilated, that on the right side slightly mobile, the left altogether motionless, and vision in the left eye entirely lost; with the right, he can still distinguish objects, though with difficulty, and without a correct appreciation of their size, form, or colour.

A year and a half has elapsed, since the patient, habitually employed in writing, has been compelled to relinquish his avocation. During the last eight years he has been subject to violent headaches.

Countenance pale; lymphatic temperament. Prescription—*venesection, seton to the neck*,—slight melioration. Blisters were subsequently applied upon the forehead and temples, and the improvement progressively advanced to such a degree, that at the expiration of three weeks, the patient, from the bridge of the Hôtel Dieu, could see distinctly the passengers walking upon the *Petit Pont*.

The same measures were continued, less actively however, and the patient remained in the same condition till the 13th November. On the day just mentioned, he complained of cephalalgia, and acute pains in the eyes and ears, which appeared to converge towards the middle of the head.

On the 15th, the cephalalgia and pains continuing, fifteen leeches were applied behind the ears, which removed the headach, but neither this, nor any other measure that could be devised, had any effect in mitigating the pains.

On the 21st, at the usual hour of visit, two or three minutes after having spoken with me, he died suddenly, without a groan, or any change in his position.

Autopsis.—In the space between the junction of the optic nerves and the pons varolii, within the vessels which form the arterial circle, a cyst was found about the size of a

small hen's egg, with its parietes partly fibrous, and partly osseous, particularly on its anterior and superior portion, where it corresponded with the junction of the optic nerves. The cyst was filled with a yellowish matter mixed with blood, nearly one-third of which was solid and of a tuberculous appearance, while the remainder was liquid and oleaginous.

Laterally and superiorly the cyst corresponded with the optic nerves, which were flattened and almost destroyed. That which remained of these nerves, adhered on the inner side to the cyst, by means of the remains of altered cerebral substance, and terminated anteriorly, in a white point, upon the osseous portion corresponding with the commissure. Farther onwards, the nerves were seen wasted, and pursuing their course towards the orbit, but between this anterior and the posterior portion, there was no other continuity than that formed by the osseous parietes of the cyst. In the eye, the retina was observed thin, of a reddish colour, and almost transparent. Nothing remarkable was observed in any other part of the brain.

There was no trace of the pituitary gland, its situation being entirely occupied by the cyst.—Sanson, chirurgien; Eus. Corbin, interne.

There are two facts related in the preceding case, so as to leave no doubt of their reality. *The patient saw objects distinctly a few days before his death*, and there was a complete interruption of the optic nerves, in their passage from the eye to the brain. In fact, it is stated, that *there was no other continuity between that portion of the optic nerves situated anteriorly to their commissure, and that situated posteriorly, than what was formed by the osseous parietes of the cyst*. Add to this, the atrophy of the optic nerves, and the thin and transparent condition of the retina, circumstances which are only observed in chronic derangements of vision; moreover, we often see persons who have been blind for a number of years, and in whom the optic nerves have undergone no diminution of volume, or any perceptible alteration in their central medullary portion. The same thing is observed in mammiferous animals, who have accidentally become blind, or lost an eye, and in whom no appreciable alteration of the nerves can generally be detected.* Now, admitting the truth of the circumstances related, it is not possible in the case above mentioned, to refer the existence of vision to the optic nerves, since they were evidently incapable of exercising their functions, and we must therefore have recourse to the fifth pair, in order to conceive how the impression of the

light could be communicated to the brain. I am aware, that this conjecture will appear very singular to the majority of readers, accustomed to consider the optic nerve as equally essential to vision, as the heart to the circulation; but if it lead to researches which may overthrow or confirm it, it will, in either case, not have been uselessly related, the rather, that it is only in the latter view, that I attach any importance to it. To limit oneself to conjectures, is to do nothing; while to endeavour to verify them by means of observation and experiment, is to labour advantageously for the advancement of science.

I ought to observe, however, that my experiments on the optic nerves,* are not favourable to this supposition; for if one of these be divided anteriorly to its decussation, vision is destroyed in the eye of the injured side; but if it be divided posteriorly to this union, then the eye of the opposite side loses its action; while if the decussation itself be divided from before backwards, in the median line, the animal remains entirely blind. It follows, therefore, from these experiments, that the optic nerve is indispensable to vision; but here the lesion is sudden, while in disease, it takes place gradually and slowly. Is this difference in the *modus operandi* of the cause, sufficient to account for the extreme difference in the effect? Before pronouncing a decided opinion, let us await farther information, and suffer no opportunity of acquiring it, to pass unimproved away.

I will subjoin, to what has been already said, the note of a case, taken in my hospital, and which has been preserved only by reason of its singularity.

Hôpital de la Salpêtrière, 4th July, 1827. The body of a woman, seventy years of age, was brought to the amphitheatre; she had died of an affection unconnected with the nervous system.

She had long been blind of the right eye, the crystalline of which was opaque; vision in the left had been preserved.

The optic nerve of the blind side, was of its ordinary volume; that of the other, was much smaller, attenuated, and wasted. The atrophy was not perceptible beyond its decussation.

This alteration was so evident, that the physicians present at the dissection, when they had seen the nerve, supposed the blindness to have existed on the left side.—Guillot, élève interne.

I much regret not to have myself witnessed this circumstance, and also, that farther information was not obtained before the death of the subject; but such as it is, it will, I hope, induce physicians who have occasion to examine the bodies of individuals, partially or wholly blind, to give particular attention, not only to the condition of the optic nerves, but also to that of the fifth pair. The most guarded deduction that we can make, from what has been

* In birds, the loss of sight is followed, in less than fifteen days, by atrophy of the optic nerve and tubercle, with the disappearance, more or less completely, of the medullary matter of the nerve, of the pearly envelope of the tubercles, and of a part of the proper substance of these eminences.

* Journal de Physiologie, tom. 4.

already stated, is, that we have still much to learn respecting the functions of the nervous system.*

From the London Medical Gazette.

CASE OF PARALYSIS OF THE LOWER EXTREMITIES, COMING ON GRADUALLY—Fatal. [La Charité.]

A hackney-coachman, between 35 and 40 years of age, above the middle stature, and enjoying good health at the time, when in a state of perspiration, walked with naked feet on a marble floor; the perspiration instantly ceased, and was followed by cold chills, and a sense of uneasiness during the whole day. The following day he complained of sore throat, which was relieved by sudorific drinks, but lassitude, want of appetite, and some fever remained, though without headach. Afterwards the lower limbs swelled, the patient experienced in them a feeling of formication, and soon afterwards they refused to sustain the body. The toes presently became the seat of a sensation of coldness, which was propagated to the legs, thighs, buttocks, and at length to the lower part of the loins. A physician was called, who, looking upon this as a rheumatic attack, applied leeches to the insteps, and frictions, with a sedative balsam. The patient was admitted into La Charité on the 1st January, twenty days after the suppression of the perspiration. He was then in the following condition: the features expressing great suffering; the lower limbs in a state of complete paralysis; he complains of pricking pains, especially in his feet; they are perfectly helpless, and fall down like dead masses when lifted up by the hand, but they preserve their sensibility. Within the last two days only, the upper extremities have begun to take on the same disease as the lower ones; at first the fingers, then the fore-arms, lost the power of motion, but the paralysis is not complete; the patient is able to extend and bend them in a very limited degree; the arm itself is moved with rather more facility; nevertheless the muscular contractions are very feeble, for he is not able to keep it in an elevated position—it falls in the same manner as do the lower limbs: otherwise, sensibility is not destroyed in any part of the body; the pulse is frequent; the face and neck perspire; the paralysed parts are dry; the respiration is per-

formed by the diaphragm; the patient is oppressed; his voice altered; he tries from time to time to make long inspirations, for the chest can scarcely be said to dilate. There is constipation, scanty urine, but no headach. The intellectual faculties are perfect. *Four bleedings from the arm, blisters to the legs each day, a potion with gum and three ounces of castor oil, strict diet and emollient drinks*, were the means employed between the day of his admission and the 7th of January. Up to this last day the patient had not had an evacuation; the oppression not only still continued, but was increased by a great quantity of mucus which choked up the bronchi, and which he could not get rid of without great exertion, the efforts of the cough were so feeble. The pulse was 110, and weak; the perspirations continued and were very copious, especially during the night. *Julap with sirup of poppies, barley water with honey, blister between the shoulders.* Three copious stools were procured by a purgative clyster, and an ounce of castor oil.

It is needless for us to follow up the daily reports further: the patient gradually sunk, and died on the 21st.

Examination.—The dura mater covering the brain was healthy, and there was but little serum under the arachnoid membrane, which presented some opaque patches on different points of the convexity of the hemispheres of the brain. The pia mater was rather vascular, uniformly red on the posterior lobe of the right side; there was a soft gelatinous exudation in the cellular tissue which unites this membrane to the cerebral face of the arachnoid, on the convexity of the anterior lobe of the left side; the convolutions of the brain were very unequal, some small and hidden, as it were, by the larger, which rose several lines above their level; the ventricles contained very little serum; the substance of the brain, cerebellum, and tuberculum annulare, presented no remarkable deviation from health, either in consistence or colour. The spine, opened the whole of its length, presented nothing particularly deserving of notice outside the dura mater; the arachnoid appeared healthy throughout its whole extent; in the cervical region, it contained one or two spoonful of turbid serum, but it was not possible to ascertain whether this had been originally thrown out in the cavity of this membrane or not. It is probable that it might have proceeded from a large opening that had been made in the internal layer of the arachnoid, in the endeavour to lay open the spine, and that it had been exhaled in the canal discovered by M. Magendie. The vessels of the pia mater were a little injected, especially towards the lumbar region. The spinal marrow, of its natural dimensions, presented no traces of compression externally, nor any alteration of colour; divided throughout its whole length, it appeared to possess its natural consistence in the cervical portion, but lower down it was evidently softened. This softening, but slightly marked at the upper part, became more so

* In a note appended to an abstract of the preceding paper, the editor of the Lancet observes, "We do not see how either of the above cases goes to prove, that the power of vision is not dependent upon the optic nerves; in the first case, the optic nerves were not completely destroyed; and in the second, although vision was more perfect on the side where the optic nerve was affected, than on the other where it was not, still there was a very apparent cause in the right eye, perfectly independent of the nerve, viz. opacity of the lens."

towards the lower extremity; it was greater in the centre than at the circumference, but it was not more red, or vascular, than in the healthy state.

The organic alteration produced by diseases of the nervous system are not always appreciable; and even when they are so, they are sometimes so little marked as to be easily overlooked, or mistaken. In these organs, the smallest alteration may produce the most fatal effects; and if the examination be not made very minutely, these alterations are not discovered, and then we have recourse to the sounding terms of *vital lesion*, or *functional lesion*, to explain the morbid phenomena. Such was the state of the case just recorded. The spinal marrow, and its membranes, were in a healthy state; it was of its natural size and colour; when divided throughout its whole length, it presented neither infiltration nor injection of blood; it was declared to be perfectly sound—when a more attentive examination proved that its consistence was not alike in the whole of its extent.

It is evident in the above case, that the softening of the dorsal and lumbar portions of the spinal marrow was the principal cause of the morbid phenomena, and of the patient's death; and that the appearances found in the brain were neither the primary nor most important disease. The progress of the paralysis indicates the course of the affection of the spinal marrow; for some time neither the functions of digestion or respiration were troubled; there was but little if any fever; the patient felt merely lassitude and weakness in the lower limbs, but still was enabled to follow his usual avocations. These symptoms showed that the disease was then confined to the lower part of the spine. Afterwards, loss of appetite and constipation came on; at the same time, respiration became difficult, the contractions of the heart became more frequent, and, lastly, the upper extremities became affected in the same manner as the lower limbs. What could be the cause of these disturbed functions, if not the change in the spinal marrow propagating itself by the continuity from the lower to the upper part?

If we may judge by the anatomical appearances, the inflammation can never have been violent; it may be doubted whether it existed at all, since M. Recamier considers these softening as of a peculiar nature, independent of inflammation. The sensibility was preserved entire, and yet it did not appear, upon examination, that the posterior cords of the spinal marrow were less changed than the anterior. This strengthens the mode of explanation given by M. Lallemand of the experiments of M. Magendie, as to this point of physiology.

The most important phenomenon of this disease was the disturbance in respiration. The palsy of the intercostal muscles, by preventing the free dilatation of the chest, by degrees impeded the change in the blood, and brought on asphyxia; this was also hastened by the quantity of mucus accumulated in the bronchi, owing to a defect in the powers of

expulsion. Thus the patient was a good deal relieved when, by proper methods, the respiration was rendered more easy.

From the Bulletin des Sciences Medicales.

CONGESTION SANGUINE DE TOUTE LA SUBSTANCE CEREBRALE, ET SQUIRRHE DES LOBULES ANTERIEURS DU CERVEAU. Par M. le docteur V. RAMBELLI.

Count Annoni, æt. 55, of a sanguineous temperament, had from infancy been subject to epileptic attacks; he had moreover, at certain periods, palpitations of the heart, which induced the fear of the existence of serious lesion of that viscus. Having made use of the sea bath for the cure of a dartrous affection, with which he had been long troubled, he began soon after, during the month of October 1824, to complain of pain in the head, which returned particularly after meals, and was accompanied with somnolency. A blister was directed to the neck, and frictions with tartar emetic ointment upon the parts which had been the seat of the dartrous affection. This treatment proved totally ineffectual, and the disease continued to advance. On the 12th of February, 1825, he fell down in a state of syncope, which continued about half an hour, when he revived, addressed some questions to the assistants, and slept for the space of forty or fifty minutes. Dr. Dell'acqua being called, supposed the patient labouring under debility of the nervous system, for which he directed the sulphate of quinine, a good diet, and a small quantity of wine. No beneficial consequences followed this treatment, and Count Annoni, who, for an indeterminate period, had been insensible to the action of the sun, (*insensible à l'insolation*), and in whom the act of sneezing had, for several months past, occasioned an acute pain at the root of the nose, soon presented the following symptoms; pulse full, elevated, and strong; digestion very regular and energetic; face ruddy; continual pains of the head, accompanied with a sensation of heaviness in the frontal region; somnolency.

March 10, immediately after eating, he suddenly lost the use of his senses, the mouth filled with foam, the extremities were convulsed, stupor, and universal sweats supervened, and this state was prolonged more than twelve hours. A blister was applied to the back of the neck, and an electuary composed of cinchona, serpentaria virginiana, arnica, and sirup of orange bark, administered internally. The drowsiness continued notwithstanding, and was almost constant; the face was bloated; the eyes red and scintillating. A pain extended from the left side of the occipital region to the forehead, when the sensation of weight was insupportable; the patient appeared lost in a kind of stupidity; generally he was unable to answer the questions put to him, and when he did so, it was

by a simple smile; the hands trembled; the influence of the will was considerably diminished; the abdominal muscles were affected with frequent cramps, and notwithstanding, there was no derangement of the digestive functions.

In the beginning of June, six leeches were applied to the temples, the internal remedies already prescribed were continued, and the patient went into the country, whence he returned, after the lapse of a month, in a state bordering upon mental alienation. On the 5th, 6th, and 9th of July, leeches were applied successively to the epigastrium, temples, and anus. The last application was followed by syncope, and in the evening there was profound stupor; loss of sensibility; respiration stertorous, short, and weak; pulse obscure, small, trembling, and unequal. A piece of caustic potash was applied on the left of the nucha, and sinapisms to the feet. On the 10th the stupor disappeared, and the patient appeared considerably better, but memory and judgment were totally destroyed; the hands moved in an automatic manner, and the feet were incessantly agitated. Reiterated bleedings, both generally and locally, pills of calomel and scammony, blisters to the legs, thighs, and head, a mixture of the succinate of ammonia, sulphate of quinine, and the infusion of arnica, were successively directed; and eventually, after alternations of improvement and relapse, the patient died on the 29th of July.

On dissection, all the vessels of the encephalon were found gorged with blood, the cortical part was more indurated than natural, and the medullary portion presented, when divided, a great number of bloody points; the lateral ventricles, particularly the left, were filled with serum; the fourth ventricle contained a tumour of a peculiar character, of the size of a hen's egg, and very firm, which was divided into four bundles, varying in size, and irregularly knotty, having a brownish aspect, and extending towards the anterior part of the cerebral hemispheres; the two anterior lobes, that of the right side especially, were in great part converted into a very hard substance, which crepitated under the scalpel. The cortical substance which covered the tumour anteriorly, and that situated beneath it, were entirely disorganized; but the posterior superior third of the lobes, had acquired the density which was observed in the other parts of the brain, and the anterior and inferior portion of the same lobes, presented a similar disposition to the depth of several lines. The small extremity of the tumour touched the coronal suture, while the great was continuous with the bundles, which were surrounded by a medullary substance, converted to a certain depth, into a soft, translucent matter, analogous to animal gelatine. In the centre of the tumour, was a cavity about the size of a nut, the parietes of which were softened, and had an orange yellow colour; it contained a small quantity of a yellowish fluid, and presented some hard membranous bridges.

The trachea had a deep red colour; the lungs, though in other respects sound, were engorged with blood; the bronchii obstructed by a bloody foam; the two auricles contained very hard coagula, resembling in colour the buffy coat of inflammation; evident traces of inflammation were observed on the internal membrane of the aorta; the abdominal viscera were sound.

From the *Edinburgh Medical and Surgical Journal*.

CASE OF APOPLEXY FROM PRESSURE OF THE INTERNAL JUGULAR VEIN,
applied to check Hemorrhage from a wound of that Vein. By C. HEINEKEN, M. D., Madeira.

To the Editors of the Edinburgh Medical and Surgical Journal.

GENTLEMEN—As I have never met, either in practice or any popular work on surgery, (with the more recondite I confess myself but little familiar,) with a case similar to the following, I am induced to solicit its publication, not because it belongs to that, generally speaking, most useless of all classes, *the rare*, but because its result points out a very different mode of treatment from that which nine persons in ten would adopt upon meeting with it for the first time, and the adoption of which must, I conceive, inevitably prove fatal, as it did in this instance. I did not see the man while living, nor was I present at the *post mortem* examination; but I give the case nearly in the words, and at the express desire of the gentleman under whose care it occurred, and have no doubt about the accuracy of the details. I am, &c. C. HEINEKEN.

Funchal, Madeira, 25th March, 1828.

Joan Fernandez, a soldier in the artillery, was stabbed between the condyle of the lower jaw and the mastoid process, just below the lobe of the ear on the left side. The knife with which the wound was inflicted was about two inches and a half in length, about half an inch in breadth, with a slight curve at its point; and it had been plunged up to the handle. Profuse bleeding ensued, which one of his comrades stopped by pressing with his finger upon the wound, after a large quantity of blood had been lost; and he was removed to the Portuguese hospital. Upon examination the temporal artery was found to be wounded, and with considerable difficulty was secured by ligature. Upon the removal of pressure the hemorrhage returned almost as violently as ever, and in a constant full stream of the size of a goose quill. As the man was greatly exhausted by the large quantity of blood lost, the first object was to suppress the hemorrhage as speedily as possible; and with this intent the wound was filled with pieces of sponge, and compresses were applied over them. In two hours from the suppression of the hemorrhage he became comatose, with paralysis of the right arm and leg, and a full pulse. Twelve ounces of blood were taken from the arm,

an active cathartic given, and in the evening, as the symptoms remained the same, other twelve ounces were abstracted. On the following day the same plan of treatment was persevered in, and about every eighth hour more or less blood was withdrawn, but without giving any decided relief. On the seventh day he expired apoplectic.

Upon examination, a wound more than half an inch in length was found in the internal jugular vein immediately below the base of the skull, and, with the exception of the temporal artery before-mentioned, no other vessel, nerve, or part of consequence was injured. There was no opportunity of opening the skull.

It is, I think, very evident that the means had recourse to in this instance were the immediate cause of the man's death. That the pressure, by being carried to the extent of rendering impervious the canal of the vein immediately upon its quitting the skull, had produced apoplexy, which frequent and continued bleedings failed to relieve; and that, as it was absolutely necessary, by some means or other, to put a stop to so formidable a hemorrhage, the only chance of preventing the accident from proving fatal consisted in permanently reducing the supply of blood to the brain. This might have been attempted by tying the carotid artery on the same side, and, had evidence of pressure on the brain still continued, by putting a ligature around the other carotid also. In that case the operation alone would probably have had a fatal effect; but it would have been the only hope; and if, under any circumstances, a man can survive the obliteration of both these vessels, that appears to be one of the most favourable, in which, as in the present instance, the escape of blood from the brain had been prevented by the closure of so direct and important an exit. In speaking thus I would have it understood, that I do so after knowing the result,—that, had the case occurred to me for the first time, I should in all probability have treated it in the manner actually practised, and brought it to a similar termination; but there are not wanting those who may have both opportunity and skill to put to the test of practice what I have suggested as merely plausible in theory.

From the Medico-Chirurgical Review.

FEVER.—DICTIONNAIRE DE MEDECINE; ARTICLES, FIEVRE—GASTRO-ENTERITE. MM. COUTANCEAU et RAYER.

It is, we think, much to be lamented, that the noble example set by our Gallic brethren, in the publication of two successive voluminous dictionaries of medicine and its collateral sciences, should not have been followed by any similar undertaking on this side of the Channel. In a department of knowledge, as yet so essentially progressive, so mutable in its doctrines and in the manner of their application to practice, there is nothing which, in our opinion, can so effectually contribute to improvement,

as short, concentrated monographs on particular diseases, frequently given to the public, in a collected and alphabetic form, as in the work from which we select the present articles.

The usual obstacles to publication on select and favourite topics—the demand, on the part of the bookseller, for a certain quantity of matter—the apprehension, on the part of the author, that he may not be able to distend his subject to the necessary bulk, without either hiding, or disfiguring its best features, are all removed by the plan of collective labour in a dictionary, such as that now before us. Here the collaborateur furnishes a dense, perspicuous treatise, containing an epitome of every thing that has been done by others on the subject, up to the hour in which he writes, together with his own opinions, and the results of his observation and experience. The name of the writer being affixed to each article, makes him responsible to the public for his own production, and enforces, by the strongest ties we have upon mankind, their interest and fame, the necessity of making his Essay the best that he can furnish.

There is an amiable, and we conceive, an enviable peculiarity, in many of the French works of science. We often find some of their best productions under a double signature. Two friends, emulous in the pursuit of knowledge, agree to make their researches together; they assist and stimulate each other, and the result of their joint labour, is generally something superior to what either of them could have singly effected. We can boast of but few examples of this kind of literary partnership, particularly in our medical writings.

The distinguished authors of the Essays now before us, are already too well, and too favourably known to the public, to require any bibliographical notice of their works from us. M. Rayer more especially, the joint author of the first (*Fièvre*) and the sole author of the second article (*Gastro-Enterite*) at the head of this review, has given us, in his book on the Diseases of the Skin, a sufficient guarantee, as to the strength of his mind and the accuracy of his conceptions.

Although the French medical writers are generally remarkable for the precision and method with which they treat their subjects, we cannot, we fear, hold up the article *fièvre*, as a very marked example of lucid arrangement of parts. It possesses, however, many excellencies to compensate for this deficiency. The following appear to us to be the chief objects of the first monograph.

1st. Our authors consider the word *fièvre*, with its Greek and Latin synonymes, as a name used to represent one or more morbid phenomena.

2dly, They enter into "*general considerations*," on the subject of fever, and under this head they pass in review the opinions, as to its nature and cause, of the most celebrated pyretologists, ancient as well as modern, chiefly with a view to decide this important question, viz. whether the fevers of authors,

but more particularly those of M. Pinel, ought to be considered as essential, idiopathic affections of the system, or merely as "physiological expressions of some local disease?"

3dly, They give the conclusions to which their own researches and observation have led them with regard to this question, and for the pathology and treatment of the inflammations causing, or constituting, (in their opinions) all fevers, they refer to the distinct heads, *Enterite, Gastro-enterite, Meningite, &c.*

In endeavouring to open any thing like a clear path, through the dense cloud of theories that has hitherto hung over the various doctrines of fever, like mist over the Valley of Mirza, we shall take our authors as our guides, reserving to ourselves, however, the privilege of making our own arrangements for the undertaking.

1. "The word *fièvre, fever, febris*, from *fervere, to boil*—*πυρετος* from *πυρ* fire, is not, say our authors, a substantive, of which the singular is not more distinct than the plural, as some have ironically observed; it is a substantive, the plural and singular of which have been used successively in different acceptations. It is an expression which has been employed by turns to represent augmentation of animal heat—increase of heat with acceleration of the contractions of the heart—the latter phenomenon without increase of heat—a general disturbance of the functions without topical lesion—a salutary effort of nature to cure disease, or to concoct the crude humours infecting the mass of animal fluids—a primary, and general modification of the economy, sometimes producing inflammations. An acceleration of the course of the blood, by means of a quickening of the contractions of the heart, with increase of calorification, and disturbance of the principal functions, &c."

"Fevers, according to Galen,* are diseases that come on without inflammation, without abscess, without local pain, without erysipelas, without a special lesion of any part. 'If,' says this great physician, 'an inflammation of the side, or of the lungs, produce fever, these diseases do not take the name of fever; they are called peripneumony, affection of the spleen, &c.' Celsus remarks, 'that two or three symptoms are not enough to characterize a morbid state. Heat, and increased frequency of pulsation in the veins are certainly the two principal characters of fever, but alone they do not constitute it, because many operations of the economy, such as fear, anger, &c. are capable of producing them.'"

M. Pinel differs but little from Galen. The word fever with him, implies increased heat, hurried circulation, disturbed functions, and the absence of local lesion. The definition given by Selle is still more vague.

"Fever is a disease variable in its course and its duration. There is cold, and heat, with the pulse sometimes more, sometimes less

quick than natural. Cullen evidently follows Galen, when he defines fever as pyrexia without any primary local disease."

Thus we see, that by each succeeding definition of the word fever, the basis of a theory as to its nature and cause, nay even as to its treatment, is at once traced out.

Such being the vague and unsettled state of the notions connected with this word, we have deemed it necessary to declare, once for all, that when we may hereafter use the word fever, we would be understood to mean that state, in which we find heat of skin, quickened pulse, and deranged secretions, without any reference whatever to the causes of these phenomena.

2. GENERAL CONSIDERATIONS.

There is no great difficulty in finding out to what sect our authors belong, from their readings of the great koran of physic, the works of Hippocrates. The learned Hakeem, in these, as in the book of his prophet, can find shadowed out the rudiments of all that man has ever yet discovered, as to human diseases and their cure, provided that he knows how to interpret the sacred text. In fact, there has been no view hitherto taken, either in pathology or therapeutics, of which we may not detect some traces, however obscure they may be, in the writings, authentic or apocryphal, attributed to the Father of Medicine. Thus the doctrines of the very latest and most popular living pyretologists, as to the local origin of fever, find considerable support in the recorded opinions of the Coan Sage. Take the following as an example.

"In angina, there is *fever*, pain in the head, swelling of the jaws."

"In wounds of the brain, *fever* and bilious vomitings are the necessary consequences."†

Hence it is argued by our authors, that, as in the times of Hippocrates, the knowledge of pathological anatomy and physiology was so very limited, and as there were so few opportunities of connecting the symptoms during life, with the state of the organs after death, he was induced to attach to *fever*, as the chief morbid phenomenon of every acute disease, all the other symptoms that he had observed, and thus to make it a generic term, to designate all diseases accompanied by morbid heat, but to which he could not assign a local origin, as in the cases of angina and injuries of the brain, just quoted. For our own parts, we think that there is abundant evidence to show that the word *Πυρετος* was used by Hippocrates, on some occasions, to designate the febrile symptoms attending local inflammations which were palpable and visible to all, on other occasions, to designate the same symptoms more or less aggravated or complicated, but unaccompanied by any topical affection then cognizable. The leading groups of these symptoms, he naturally and properly divided into

* Edit. Chart. tom. ix. Aph. Hipp. Comment. pag. 184.

* Hipp. de Morbis, lib. ii.

† Ibid. lib. v.

different types, distinguished by the modes and succession of their attacks; such as continued, intense fever—intermittent fever returning every day, every third day, &c. We think that our authors have established, in a most satisfactory manner, that the different epithets applied by Hippocrates to particular fevers, were only descriptive of symptoms accompanying that which, with him, constituted the principal phenomenon, namely, heat of skin, and not intended to designate distinct species of the same disease. Thus, fever, with the epithet *phricodes*, means febrile heat, preceded by shivering; with the epithet *lingodes*, morbid heat accompanied by hiccup; and so of the others.

We shall now endeavour to arrange under distinct heads, in a more concise manner than our authors have done, the different opinions that have hitherto been promulgated, as to the nature and cause of fever. We shall, of course, notice only the leading theories, those that appear to have given, during their respective reigns, a marked character and direction to the treatment of disease. It would be manifestly impossible to enumerate, in the space allotted to a review, all the minor opinions which physicians formed for themselves, from time to time, made up of parts selected from the greater systems, and combined according to their own views. The principal sects of pyretologists, whose theories as to the nature and proximate cause of fever, have swayed the science of medicine from its earliest records up to the present day, might, we think, be arranged under the following heads, viz. The Humorists—The Animists—The Solidists—The Essentialists—The Localists.

THE HUMORISTS.

The earliest, beyond all question, the most universally received, the most permanent in its duration, and we might, perhaps, add, the most practically useful in its application to the treatment of disease, is that theory which was born with the healing art itself, was methodized by Galen, and is acted on by many up to the present day, we mean the *humoral doctrine*. The founder of the humoral, and, as it would seem, of many other doctrines also, was Hippocrates. He came to the conclusion, that all fevers are produced by a vitiation of the fluids. The vagueness and want of precision which we find in his writings on this subject, and his total silence as to the state of the pulse, are due to his entire ignorance of the mechanism of the circulation. He saw clearly, however, that the secreted fluids became depraved, some more palpably so than others, as soon as the fever was established. Hence his notions as to the agency of the atrabilis, &c. In short, with Hippocrates, health consisted in the existence of a perfect equilibrium in the material composition of the humours; disease in the destruction of this equilibrium; the healing art, in correcting the predominant morbid qualities of these humours.

Galen attempted to improve on these common-sense and simple notions, by attributing

to each individual fluid the capability of producing a particular class of diseases; but he only succeeded in complicating them into something bordering on absurdity. The natural consequence of a theory, which supposes a separate morbid agency in each depraved fluid, was, the suggestion of distinct remedies, or purifiers, to be used as each humour became more prominently peccant, or as a particular humoral idiosyncrasy happened to prevail. Hence the almost countless multiplicity of Galenic medicines. Each fever had its distinct origin. Thus, quotidian fever was produced by the putrid pituita; tertian by the yellow, and quartan by the black bile.

These doctrines, after they had been lost in the dark ages, were again brought up by the Arabian physicians, but so mistified and complicated, as, for a time, to wear the appearance of novelty. They continued, however, to regulate the curative administrations of medicine, with the most uncontrolled sway, for many centuries.

THE ANIMISTS.

The first important schism from Galen's creed, was that of Paracelsus and Vonhelmont, the Mahomet and the Ali of physic. The former of these enthroned a demon, which he called Archæus, in the cardiac portion of the stomach, and, under his orders, he placed myriads of inferior Archæi, in the different organs of the body. All the functions were regulated by these, armed with their respective *ferments*. All diseases, and consequently fevers, were produced by the rebellious and improper use made by the minor Archæi of their respective ferments, and by the efforts of the great Archæus to bring them into subjection and order. Vonhelmont substituted, in his theory, the rational soul for all the Archæi, great and small. According to these theories, as each disease was looked upon as a battle, fought upon a regular plan, between the Archæus and the ferments, or between the rational soul and the morbid matter, the physician had little more to do than to look on. To these extravagant notions we owe that famous work by Stahl, "*Ars Sanandi cum expectatione*." Hence the *médecine expectante* of the Essentialists of the late French school.

Although the archæus and the rational soul, (the *enormon* of Hippocrates and Plato revived) soon fell into disrepute, the notion, that fever is the result of a salutary effort of Nature to expel a ferment from the blood, was entertained by the great Sydenham himself, and is obscurely seen in the "*vis medicatrix*" of Cullen.

The medical theories of the Alchemists and their followers consisted in the belief of certain rude chemical actions going on amongst the fluids, and directed their efforts to the discovery of alexipharmacs, panaceas, and elixirs of longevity, to be substituted for the cumbersome farrago of Galenic simples.

The Mechanists, at the head of whom we may place Boerhaave, rejected with disdain the humoral doctrines, as far as the influence

of the recrementitious fluids was concerned. They retained, however, all that was good in the views and practice of Hippocrates, and pretended to direct their own upon principles purely mechanical and mathematical. The volume and form of the globules of the circulating fluids, as compared with the capacity of the vessels containing them, formed the basis of their theories as to the cause of disease.

THE SOLIDISTS.

Cullen may be placed at the head of these, for Hoffman was but half a Solidist. This school attributes all fevers to a spasm, or morbid contraction of the extreme capillaries. They moreover confine the term to acute diseases, unaccompanied by any primary local affection. The doubts which this theory involves, as to whether tone ought, in certain cases, to be given or taken away, naturally led to a vacillating practice, producing little or no influence on the progress, or result of the disease. But a still more mischievous effect was produced by this doctrine. It arrested the progress of medical improvement by an artificial classification of diseases, entirely unsupported either by clinical observation, or pathological anatomy. John Hunter and his followers, when they use their favourite language, "the action of the vessels of a part," only mix up the solid parts of this theory with the immaterial vitalism of Stahl and Vonhelmont.

THE ESSENTIALISTS,

Or, more properly, the Ontologists of Broussais, having Pinel for their chief, finding that they could not maintain the independence of all fevers upon local inflammation, limited the number to five, which they called essential, viz. inflammatory, bilious, mucous, adynamic, and ataxic fevers. They held that the entity, or being, of which they appear to know nothing, but which they call fever, produces, by a mechanism which they do not pretend to describe, all the symptoms and organic lesions which we witness during the progress of these diseases, or find after the death of those that perish by them. This, in fact, goes to assert that fever is fever, that it will have its fling in spite of all we can do, and leads once more to the practice of the *médecine expectante* and harmless ptisans. The bitter enmity, which our authors every where manifest towards the theories of M. Pinel, renders the following homage to his candour and integrity doubly valuable:—"A faithful interpreter of facts, even when in opposition to his theoretical principles, he attributes the ataxic fever to a morbid state of the brain. This was the first step in that road, which was afterwards destined to lead to a more important reform."

THE LOCALISTS.

The gradual improvement and diffusion of anatomical knowledge, the more accurate and more frequent observation of post-mortem appearances, in fact, the general spread of

science, had been long preparing the basis of a much more important theory of fever than any that had been hitherto substituted for the doctrines of Hippocrates and Galen. The Localists, or those who hold that all fevers are caused by local disease, owe the materials of their theoretical edifice to the dissections made by Bertholin, Bonetus, Morgagni, Baglivi, Chirac, Prost, the Hunters, Bichat, and a host of others. The traces of inflammation or congestion, almost always found about the encephalon and abdominal viscera of those that perish from fever, very early induced different pathological anatomists to consider one or other of the varieties of these local affections as the proximate cause of fever. Chirac conceived that fever is owing to inflammation of the brain. Baglivi says, that the fevers called malignant are produced by phlegmon, or erysipelas of the intestines. M. Broussais, however, appears to have been the first who digested the facts collected up to this day, as to local irritations and inflammations, particularly those of the stomach and bowels, into a regular code of doctrinal pathology. Our authors, zealous supporters of his doctrine in all its ramifications, thus introduce this celebrated pyretologist to our notice.

"Penetrated by the sublime views of Bichat as to the sympathies, rich in numerous facts observed with a rare sagacity, M. Broussais came to overturn, from the very foundation, the antique edifice of fevers. In his works, as well as in his lectures, he has applied himself, for many years, to demonstrate, that the fevers which had been called essential, were nothing more than local diseases, inflammations, nay, even *gastro-enterites*. The following propositions (say our authors) may be looked upon as the summary expression of his doctrines.

"1mo. Fever, considered in a general and abstract manner, is never other than the result of a primitive or sympathetic irritation of the heart, through the effect of which this viscus hurries its contractions.

"2do. Every irritation, sufficiently intense to produce fever, is an inflammation.

"3tio. All the fevers of authors are connected with *gastro-enteritis*, simple or complicated. This they have all overlooked when unaccompanied by local pain, and even when accompanied by pain, they consider it as an accident. Authors have sometimes said, that certain fevers depended on inflammation of the digestive organs, but they have never said that these fevers, the pretended essential fevers, cannot have any other cause; never that they were produced by the same mechanism as the fever of peripneumony; never, in short, that there are no essential fevers.

"4to. It is by *gastro-enteritis* that small-pox begins; by *gastro-enteritis* and an acute ocular, nasal, guttural, or bronchial catarrh, that measles and scarlatina commence.

"5to. Intermittent and remittent fevers are periodical *gastro-enterites*, but the encephalon and other viscera are irritated sympathetically, as in continued fevers, and may also become

the principal seat of irritation, and put on inflammation in an acute or periodic manner.

"6to. The fevers termed pernicious differ from other fevers, only by the violence and danger of congestions."

M. Broussais holds that these propositions are proved by the following facts; we give his own words.

"1 mo. All the causes of fevers act locally; they all irritate the gastric mucous membrane, that point of the organism on which the action of every morbid cause strikes.

"2do. In almost all fevers there are unequivocal local symptoms of irritation of the stomach and small intestines, a fact that does not allow us to overlook the existence of gastro-enteritis.

"3tio. The sympathetic symptoms, in the absence of symptoms of gastric irritation, demonstrate evidently, though indirectly, the existence of gastro-enteritis in all fevers.

"4to. A great number of organs do not participate in a morbid state in fevers, and of those that do, some are more, some less affected by it.

"5to. The adynamic and ataxic symptoms are due to irritation.

"6to. After death we always find traces of gastro-enteritis.

The above assertions constitute the doctrinal code of the Broussaists, certainly the most important medical sect of the present day.

The curative administrations in all fevers, practised and inculcated by this, as, by all other sects, are the legitimate offspring of their theory, as to the proximate cause of the disease.

We do not mean to notice here, the minor varieties of Localists of our own country, (schismatics from this parent stock,) neither those who found their innocent little heresies on particular symptoms and traces of organic lesion, not very frequently present; nor those, whose vague notions as to topical congestions, have led to destructive, and perhaps, erroneous practice.

We pass over in silence the individual examination of M. Pinel's five essential fevers, and the arguments adduced to prove, that they are but so many examples of gastro-enteritis. Indeed, our authors can see nothing but their favourite "*gastro-enterite*" in the putrid fevers of Pringle, the typhoid epidemics, described by Fracastorius, Tissot, Roederer, &c.; nay, the very intestinal worms, often found after death, are, according to M. Broussais, produced by *gastro-enterite*.

Severe and peremptory as the Animists were in their condemnation of the doctrines of their predecessors, the Humorists—the Solidists in their enmity towards the Animists; we shall find that Localists are equally, if not more determined in their condemnation of their immediate masters and rivals, the Essentialists. The following ultimate, and sweeping conclusion, to which our authors arrive, respecting the theories of the Pinel-school, will, we think, justify this assertion.

"We conclude from the researches which

we have made preparatory to writing this article, and from the facts which we have accumulated in its different paragraphs,—that the general descriptions of inflammatory, bilious, mucous, adynamic, and ataxic fevers, arising from an attempt to reconcile facts dissimilar, or as yet undetermined, being inexact, arbitrary, and false, ought never hereafter to be reproduced in works of pathology."

As the size and expense of the work, from which we have selected the present article for review, must render it inaccessible to many of our readers, we have taken much pains to offer a concentrated view of the different theories, in the examination of which, our authors have given proofs of great research and critical acumen. We have been more particular in our notice of the doctrines of M. Broussais, because they exercise, at present, a paramount influence on the whole medical practice of the Continent; and, because they are rapidly gaining ground in this country. Theories are important, not so much from the reality and consistency, or utter groundlessness of the facts, upon which they appear to repose; as from the decided bent they have at all times given, when adopted generally, to the measures by which we endeavour to prevent, or modify disease. We have followed our authors in their theoretical research, in order that we might arrive, with minds better prepared than they otherwise could have been, to the discussion of the practical parts of our subject, the anatomical characters, the symptoms, and treatment of these inflammations, which, by some, are thought to cause, or constitute fever; by others, to be but secondary effects of a more general cause. Be this, however, as it may, it is not the less true, that if we can remove the phlegmasia, or congestions alluded to, we shall very rarely fail in curing the general functional disorder, by which they are always accompanied.

GASTRITIS, GASTRO-ENTERITE, &c.

As the admiration which our authors, (particularly M. Rayer,) every where express, for the doctrines and practice of M. Broussais, appears to us to border, rather too closely, on enthusiasm, we have deemed it prudent to take in a little practical ballast from M. Chomel, on the subject of *gastritis*, before we approach the stormy ocean of *gastro-enterite*.

"Gastritis, or inflammation of the stomach," says this enlightened physician and laborious observer, "is one of those affections, which, for some years past, have particularly engaged the attention of medical men, and given rise to a great number of works, for the most part polemic. If discussions were of a nature to throw much light on the history of disease, our knowledge of the one in question would have made rapid progress. But the experience of ages has proved, that the sciences of observation march with facts; and that scholastic disputes have seldom profited them much. Instead of collecting with care, and without any other intention than that of coming at the truth, numerous examples of

this disease, the greater number of physicians have devoted themselves to theoretic discussions, have almost entirely neglected to collect facts, or have viewed them only through the dangerous prism of prejudice. What has been the result? Inflammation of the stomach, embarrassed by an almost infinite number of useless writings, obscured by inexact, or truncated observations, is at this moment, one of the phlegmasiæ, of the symptoms and anatomical characters of which, we have but an imperfect knowledge. This assertion may appear false, to some, but will not surprise those who have carefully watched the sick, and opened many bodies. In fact, if on the one hand, it is often impossible during life, to determine with certainty, what the state of the stomach will be after death; and if, on the other hand, the anatomical examination of this viscus is not generally sufficient to determine with precision, whether there were, or were not symptoms of gastric inflammation during life; are we not compelled to acknowledge, that this phlegmasia is one of those, our knowledge of which, is as yet but very imperfect. The number, and the discrepancy of the writings, of which this disease has been the subject for many years, would be almost enough to demonstrate this assertion.

"We do not, however," continues M. Chomel, "pretend to say, that gastritis is at all times, and in all its degrees, an obscure affection; we shall see on the contrary, that in many of its forms, it is marked by symptoms as defined as those of pleurisy and pneumonia."

GASTRO-ENTERITE.

We pass over the remainder of M. Chomel's excellent monograph on gastritis, for the purpose of entering at once upon the legitimate domain of M. Broussais.

Nothing can be more lucid than the order, nothing, in our opinion, better judged, than the method, observed by M. Rayer, in his distribution of the heads, or points of view, under which he has treated this celebrated disease, or rather this *panto-pathological* essence of all diseases. The whole article is a perfect model of what a medical monograph ought to be. There is neither the laboured verbiage of speculative theory, nor the fatiguing minuteness of narrow observation. There is enough, however, from which to stock the mind with the most useful parts of what is known on this interesting subject, and enough to stimulate it to further inquiry. In short, we conceive it to be the very best, and most practically useful essay, that we have as yet seen to emanate from the school of Broussais.

The following paragraph contains some statistical details, curiously indicative of the influence of medical theories.

"Gastro-enteritis—a name proposed by M. Broussais, to designate the simultaneous or successive inflammation of the stomach and small intestines, is, of all the phlegmasiæ the most frequent, and, at the same time, that which has been oftenest overlooked or mistaken. You will not find it indicated in any

nosological table. Gastritis itself was not long ago pretty generally looked upon as a very rare disease; for of twenty-eight thousand, two hundred, and ninety-nine sick admitted into the civil hospitals of Paris, in 1807, *six only* were designated in the definitive returns, as labouring under inflammation of the stomach; whilst six thousand, one hundred, and forty-three, were treated for continued or remittent fevers. Such a result cannot be comprehended, except by admitting that, according to Brown's principles, the name gastritis was only given to inflammations of the stomach, produced by poisons; or, perhaps it may be better to suppose with Hufeland, and many French physicians, that, in consequence of some occult changes having taken place in the constitution of the atmosphere, the bilious, mucous, and adynamic fevers have been supplanted in these latter times, by gastro-intestinal phlegmasiæ, the frequent appearance of which is now no longer disputed."

M. Rayer, after showing that the specific and separate denominations, *gastritis* and *enteritis*, have been often vaguely applied by nosologists, in their artificial arrangements, to the simultaneous, or immediately successive inflammations of the stomach and small intestines, observes—

"That as by far the best authenticated part of the history of the phlegmasiæ of the digestive organs are the changes they undergo, it must be by these changes that their inflammations can be best characterized and specified." "I am (says our author) certain, that I shall be able to render more intelligible the variety of phenomena produced by these inflammations, by beginning the description of gastro-enteritis with the alterations that constitute it."

Previously to a separate enumeration of the various alterations occurring in gastro-enteritis, our author makes the general remark, that—

"All the forms, and all the terminations of acute and chronic inflammation, observable on the external teguments, or on the different points of the gastro-pulmonary mucous membrane, may take place in the stomach and small intestines, but with unequal frequency. These gastro-enteritic phlegmasiæ, noticed by Morgagni, Prost, &c., and very recently pointed out by M. Broussais, have furnished materials to Andral, Bichat, and Louis, for novel and important observations."

According to his proposed plan, M. Rayer commences with the organic alterations, (of which he describes twenty-one,) that have been found in the stomach and bowels of those who are supposed to have died whilst labouring under gastro-enteritis. The changes that occur in the colour of the mucous membrane are first considered.

Accidental red tints, which he compares with cutaneous exanthems, are said to be the most constant of all the effects of this phlegmasia. They appear either under the form of *vascular arborescence*, or of circumscribed

patches, varying from the most intense red to the deepest brown. Sometimes there are a number of little red spots, like the marks in purpura, occasioned by minute but separate submucous hæmorrhages.

All these changes of colour are most frequently found at the bottom, and great curvature of the stomach, and on the intestinal valvules. An universal deep violet tint of the mucous membrane, black ecchymosed spots, and sometimes effusions of blood into the cavity of the stomach, may, we are assured by Morgagni, be produced by the course of the blood in the vena portæ being obstructed. The slate-coloured gray, the greenish-gray shades, which M. Broussais has sometimes remarked, affecting the gastro-intestinal mucous membrane, he considers as products of inflammation; nay, the morbid dead-white paleness of the coats of the stomach and small intestines, noticed by Bonetus, but more particularly remarked by M. Guersent, as frequently found in children, is set down to the account of inflammation by our author; as also the softening, to the consistence almost of boiled starch, described by this writer.

Here we would take the liberty of remarking, that if every shade of colour, from the darkest red to the deadliest white, may give proof of inflammation having existed in the tissue where there are found, we would say, "*nimum ne crede colori.*" The necessity of this caution we have seen frequently exemplified by that accurate pathologist Lennec. In his post-mortem examinations, he would order a portion of a healthy artery, say of the aorta, to be detached, and filled with the gore of the subject. At his next clinical lecture he would order this preparation to be exhibited, when the lining membrane of the artery was always found stained or dyed a lively red, such as could not at the moment be either rubbed or washed off.

Softening (ramollissement) of the stomach and small intestines; "very rare (says our author) in the fœtus and the old; frequent in new-born infants and at the period of weaning; often observed in adults, more particularly in lying-in women. This disease attacks indifferently both sexes." By John Hunter it was attributed to the solvent action of the gastric juice; but is now pretty generally allowed to be the result of inflammation. This view seems to be confirmed by the pustules and figured inflammatory patches that generally co-exist with this singular affection, and by the success of antiphlogistic means in its treatment.

"This disease (says M. Chomel) is always accompanied by acute pain in the stomach. To this organ the patient refers all his sufferings. Can we then, hereafter, assert, that the mucous membrane of the stomach is insensible? that it expresses its sufferings only by sympathetic phenomena? Is it not, on the contrary, evident, that it obeys the common law, and indicates its lesions, like other organs, by pain, and the disorder of its functions?"

We would strongly recommend to our readers the recent work of M. Louis on this very interesting subject.

Ulcerations—a frequent termination of inflammation in most parts of the body, and certainly the most unequivocal testimony, that the tissues in which we find it after death had been inflamed during life. The greater frequency of ulcerations in the lower portion of the ileum is attributable to the greater frequency of pustulous inflammation in that part of the intestine, and not, as has been asserted by some writers, to the greater abundance of mucous follicles. "The duodenum, so plentifully furnished with follicles, is, of all parts of the small intestines, the least liable to ulceration."

Gangrene is an exceedingly rare termination of gastro-enteritic inflammation, notwithstanding M. Broussais' assertion, that he had often observed it. Our limits will not allow us to do more than merely to enumerate the other alterations which pathological anatomy is said to have found in this disease. These are thinning of the parietes of the intestinal canal; thickening of ditto; diminution, or augmentation of caliber; pustules; vesicules; papules; membraniform exudations; cream-like exudations; inflammation of the submucous cellular tissue; emphysema; œdema; perforations; vegetations; scirrhous and medullary matter; tubercles, &c.

The first stage of gastro-enteritis consists in a mere sanguineous injection of the mucous membrane. The second in any of the various morbid exudations—the third in ulcerations—the fourth in thickening, or thinning of the coats, and the formation of accidental tissues. The fifth, in gangrene or destruction of the parietes themselves. As the first, second, third, and fifth stages may be developed within the first four days, they are said to belong to acute gastro-enteritis. The fourth stage, or deep and permanent alterations of texture, belongs exclusively to chronic gastro-enteritis.

Acute Gastro-enteritis.—All ages and sexes, from the fœtus in utero to the centenarian, are liable to this disease; and every thing that can be introduced into the stomach, from the viands of the alderman's feast to the most acrid poison, from tonic draughts and emetics to knives swallowed whole, may produce it. Starving, itself a principle remedy for this disease, when carried to excess, gives rise, according to J. Hunter, Dumas, and Magendie, to inflammation, and even ulceration of the stomach.

"But of all the causes of this malady, the inflammation of other organs is the most remarkable, and certainly the least noticed. Dupuytren, Abernethy, and Broussais, have successively pointed out the influence of large burns, inflamed wounds, &c. in exciting gastro-enteritic inflammation."

Symptoms.—Of the four local phenomena usually assigned to inflammation, viz. pain, heat, redness, and swelling, only the first two are appreciable during life in this disease;

and they are both generally in proportion to the intensity of the inflammation, except when complicated with cerebral affections, and then they are never so sensibly felt. Pressure on the region of the epigastrium, seldom, or never fails to cause pain, when gastro-enteritis is present, but we should take care in applying it, not to mistake what may be the effect of our own exertion, or the patient's natural sensibility, for the morbid tenderness of an inflamed organ. This particular phlegmasia we may say, is never found uncombined with other important derangements, as has been satisfactorily proved by the dissection-returns drawn up at the Hôtel Dieu, by order of M. Dupuytren.

According to the school of Broussais, it is to the various combinations, or groupings of the symptoms attendant upon this disease, in its different stages and complications, that we owe the countless variety of fevers described by authors.* We cannot help acknowledging that there is much truth in this observation. When the constitutional symptoms common to all inflammations were present, with a white tongue and viscid saliva, 'twas the mucous fever, with a yellow tongue, the bilious, with a black, the putrid fever. When there was tenderness of the abdomen, with much debility, it was the adynamic fever. When the gastro-enteritic inflammation was complicated with cerebral symptoms, it was the ataxic; with thoracic symptoms, it was the mucous catarrhal fever, or the pneumonia notha.

If it be true, and we believe it is as good as proved, that all the symptoms known to accompany European fevers at least, also accompany the disease we are treating of, in its acute form—if it be true that in nine-tenths of these fevers there are evident signs of gastro-enteritic inflammation during life, and that unequivocal traces of its having existed are found after death—if it be true, that this connexion of fever with the phlogosed state of the digestive organs was not efficiently noticed before the time of Broussais, then must it be also true, that this pathologist has conferred an incalculable benefit on the healing art, by calling the attention of those that exercise it, to this important, and hitherto unknown coincidence.

It has been proved beyond all contradiction, by the most ample statistic records of post-mortem examinations at the Hôtel Dieu—by the experiments of Orfila, by Abernethy, and many other authorities, that large inflamed wounds, burns on the trunk or extremities, and poisons applied to the surface, produce inflammation of the gastro-intestinal membrane, of the thoracic viscera, of the encephalon. The successive effects of this transition of inflammation, whether it be by sympathy, irritation, or absorption, from the surface to the centre, are an arrest, or alteration of chylification, depraved secretions, tenderness of the epigastrium, nausea, quickened circula-

tion, heat, loss of appetite, emaciation, and all the complicated derangement of function, that extensive organic lesion never fails to bring with it. From these symptoms, as they may be variously combined in different subjects, under different circumstances, the closet-nosologist, or speculative essentialist, can compose any variety of fever, and establish an ideal species according as he finds mucus, bile, debility, or delirium prevail. But practical anatomy acknowledges only one disease, made up of certain phenomena consecutive upon a burn, a wound, or a capital operation.

Treatment.—In these cases, at least, there is a cause of fever anterior to gastro-enteritis. Broussais says that it is an irritation; but in his second proposition he states, that "every irritation that produces fever is an inflammation." See the effect of this theoretical hitch upon his practice. He applies leeches in every variety of fever, in all its stages, nay almost in every disease, to the region of the stomach, the thorax, the head, the extremities; pursuing the sympathetic irritation, or inflammation of *gastro-enterite* through all these different localities; and this, to the almost total exclusion of every other remedial measure, except the most rigid starvation, which he calls "diète absolue."

The above comprises the whole treatment of acute *gastro-enterite*, and consequently of every variety of continued fever. The expulsion of foreign bodies from the stomach and bowels, the recalling local inflammations that may have suddenly disappeared, or the subduing those that appear to keep up the disease by sympathy, can scarcely constitute an exception.* In the following paragraph on prophylactic treatment, M. Rayer offers some good, though perhaps not very palatable advice to old gentlemen, and bold practitioners.

"This disease will become more rare in old men, when they shall have given up their elixirs of longevity, their stomachic tinctures and pills—their precautionary purgatives. It will become less frequent with all classes of society, when physicians, more reserved in the employment of potent medicines, shall have begun to count a little less on the tolerance of the stomach."

Emetics, purges, (amongst the rest calomel,) nay, even bark, are all formally denounced. Nothing, as we said before, is left with which to combat disease, by this exclusive theory of localism, but leeches and starving. This dangerously simplified practice is not the result of, though its utility is said to be confirmed by, experience. It is the direct consequence of the third article of the Broussais creed, as already quoted, viz. "that all fevers

* Sauvages gives 150 varieties of fever.

* A costive state of the bowels persisting, without any alvine discharge during five, or even ten days, is looked upon as a good symptom, and is not interfered with by M. Broussais.

are connected with, and referrible to, gastro-enteritis."

CONTRAST OF LOCALISM WITH HUMORISM.

How much more natural, how much more consistent with the facts, of which we are best assured in pathology and therapeutics, is the notion, that some change must take place in the circulating fluids before fever can be developed? The practice that results from this theory rejects none of the *juvantia*, overlooks none of the *lædientia*. It leaves their management where it ought to be left, to the practised judgment of the competent physician. All that we have hitherto learnt by the study of symptoms and signs; all that we have been taught by the mechanical analysis after death, of the organs that appeared to suffer during life, confirm the justness of this notion, and contribute to its development. Do we not observe the fluids become depraved as the constitution becomes more generally, and more intensely involved in fever? Do we not see all the organs resume their healthy functions, as the blood throws off by sweat, by hæmorrhage, by urine, or by stool, something, which a moment before formed a part of its own stream? But the very source of that stream is in the stomach and bowels. The principle that regulates the proportion, the quality, the relative position of its molecules, is the nervous energy. That which affords due excitement to this regulating power, is the healthy blood. No man will pretend to say, that there can be fever when all the fluids are in their healthy condition, because, on the one hand, there is due nourishment of the nervous mass, on the other, due distribution of its influence. But, as the paramount laws of matter, generally, cannot be sacrificed to individual preservation, the very organs, whose office it is to prepare and convey the materials of health to the different parts of the animal machine, must, from their very structure, take up and prepare poisons under certain circumstances, or produce by their influence, when morbidly excited, deleterious changes of combination.

Every thing then that we do in disease, ought to tend towards re-establishing the purity of the fluids when it has been disturbed. The practice of the Father of Physic was founded upon this principle. He purged upwards and downwards, to clean and prepare the digestive organs, when they appeared to suffer from foulness. He produced direct diminution of the circulating fluids by bleeding. He administered demulcents, refrigerants, corroborants. He attended to diet, and encouraged critical discharges. But above all, he distinguished the cases and the stages of disease, in which the adoption of these respective plans of treatment might be advisable. He practised an art that required the highest powers of the mind for its successful exercise. The Broussaists, on the contrary, like the Brownists, have reduced the whole science to one or two unbending maxims, which they call physiolo-

gical, to be learnt in an hour, and practised without distinction; viz: that all fevers consist in *gastro-enteritis*, and their cure in local bleeding and starving. These remedies, excellent on some occasions, we have seen carried to such indiscriminate, and destructive lengths, by M. Broussais himself, that the subjects of them perished, perfect bloodless skeletons; and, instead of showing signs of an inflammation requiring such rigour, the whole alimentary canal resembled the finest oiled paper, in paleness and transparency; exhibiting perhaps, a few arborescent patches of red, at the most declining and sharp folds of the small intestines.

There is yet another disadvantage in this doctrine, besides those already noticed. Its views do not extend beyond the limits of anatomy, and this has been lately brought to a pitch of advancement, beyond which we cannot hope to see it carried, without drawing largely on the imagination. This doctrine does not direct our research towards that other great opening that leads to chemical pathology; a source from which, we venture to predict, the next great light will illuminate our science. This is an almost uncultivated field, and in the present state of our knowledge, holds out the most brilliant hopes to the zealous and qualified labourer. We should rejoice to see such men as Prout and Orfila, enter with energy upon the track of organic chemistry.

Broussais, his followers, and his opponents, have done much good. To their controversies we owe the rapid progress that has been made in the pathological anatomy of the hollow viscera—the prudent caution now observed, even in this country, in the use of drastic purges and emetics. But from Broussais alone, we have learned the full value of that symptom, so important in fever, the sensibility of the epigastrium to external pressure. It is by applying to the etiology of this, and its subordinate functional derangements, the lights of morbid anatomy, that we are sometimes enabled to guide our practice in the commencement of fevers, so as, by prudent depletory, evacuant, and dietetic measures, to shorten their course, and mitigate, or prevent their worst symptoms.

The pathology and treatment of chronic gastro-enteritis, as understood by M. Rayer, do not come within our plan. They are not necessarily connected with fever. We, therefore, pass them over, as well as the heads *intermittent*, and *epidemic gastro-enteritis*, because they would lead too far into polemic theory, and because the reader is referred to other articles of the dictionary, such as *fièvres intermittentes*, *typhus*, &c. The very titles, however, with what we have already said, will afford a tolerably correct notion of the almost unlimited extent, to which Broussais and his disciples apply their pathological pass-word. We must, however, add, that those who neglect to make themselves acquainted with the morbid alterations, the symptoms, and the principles generally, upon which the doctrines of this school are founded, want, in our opi-

nion, a large portion of that knowledge, which a good physician of the present day, ought to possess.

We shall close this article with the concluding words of M. Rayer.

"The alterations of the stomach and intestines are so varied, and so few comparative therapeutic experiments, have been, as yet, made under well determined circumstances, that it would be difficult to ascertain, even approximately, the sum of the cures which it is permitted to hope for from the antiphlogistic plan, as compared with the more active measures, such as moxas, blisters, setons, &c. In fine, if in the present day, we see but few practitioners administering medicines for the cure of *particular symptoms*, as was, heretofore, the case—if physicians no longer blindly combat *pains in the stomach* with narcotics—*dyspepsia* with bitters, elixirs, mineral-waters, &c.—*sour regurgitations* and *belchings* with absorbents—*vomitings* by anti-emetic potions—*constipation* by calomel—*diarrhœa* by astringents, it is because, since the splendid researches of Pujol, and of Broussais, it is generally acknowledged, that the inflammatory alterations which produce these different symptoms, ought alone to form the source of curative indication."

From the Edinburgh New Philosophical Journal.

ON THE SPONTANEOUS COMBUSTION OF THE HUMAN BODY.

On the 12th May, 1828, M. Julia Fontanelle read, in the Academy of Sciences at Paris, a memoir entitled, *Recherches Chimiques et Médicales sur les Combustions Humaines Spontanées*.

The observations which form the subject of this memoir are highly deserving of attention. In fact, besides the interest which they are capable of exciting from their very nature, they afford a new example of one of those phenomena, the existence of which has, in these later times, been questioned, solely because, while they are very singular and difficult to be accounted for, they are also of such rare occurrence, that they can only be authenticated by an aggregate mass of evidence, which evidence, although sufficient to induce conviction, may always be rejected by those who are prejudiced, or who do not give themselves the trouble of duly estimating their value.

Are there really spontaneous combustions of the human body? Such is the first question which the author examines, and he resolves it by the affirmative. Fifteen observations of spontaneous combustions, which he successively relates, enable him not only to establish the incontestible reality of the phenomenon, but also to make known the principal circumstances which accompany its manifestation. In summing up these circumstances, he remarks:

1. That persons, who have been destroyed

by spontaneous combustion, have, for the most part, been immoderately addicted to the use of spirituous liquors.

2. That this combustion is almost always general, but that it may be only partial.

3. That it is much rarer in men than in women, and that the women in which it has been manifested, have almost all been aged; one woman only was seventeen years of age, and in her the combustion was but partial.

4. That the body and viscera have always been burnt, while the feet, the hands, and the top of the head, have almost always escaped.

5. Although it is demonstrated that several loads of wood are necessary for reducing a dead body to ashes by ordinary combustion, incineration is effected in spontaneous combustions without the most combustible objects placed in the vicinity being burnt. In one case there was a very singular coincidence of two persons being consumed at the same time, in the same apartment, without the apartment or the furniture being burnt.

6. It is not demonstrated that the presence of a burning body is necessary for producing spontaneous combustion of the human body; on the contrary there is every reason to believe the reverse.

7. Water, so far from extinguishing the flame, seems to render it more active; and after the flame has disappeared, the intimate combustion continues to be effected.

8. Spontaneous combustions have appeared more frequently in winter than in summer.

9. No remedy has been found for general combustion, but only for partial.

10. Those who undergo spontaneous combustion, are the prey of a violent internal heat.

11. Spontaneous combustion develops itself suddenly, and consumes the body in a few hours.

12. The parts of the body which are not consumed by it, are attacked with sphacelus.

13. In individuals affected by spontaneous combustion, there supervenes a putrid deterioration, which presently brings on gangrene.

14. The residuum of spontaneous combustion consists of greasy ashes, and an unctuous soot, both having a fetid odour, which diffuses itself equally through the apartment, impregnating the furniture, and extending to a great distance.

The author then explains the two theories of combustion between which the learned world is at present divided; Lavoisier's, and that lately proposed by Berzelius. He then gives an account of the theories proposed for the explanation of the phenomenon in question.

Most authors, who have spoken of spontaneous combustions, have imagined they discovered an intimate relation between their manifestation and the immoderate use of spirituous liquors in the individuals attacked. They suppose that these liquors, being continually in contact with the stomach, penetrate

through the tissues, and fill them up to saturation, in such a manner that the approach of a burning body is sufficient to induce combustion in them.

M. Julia Fontanelle does not consider this explanation satisfactory. He founds his opinion, 1st, on the circumstance that there is no proof of this alleged saturation of the organs in persons addicted to the use of spirits; 2d, on the circumstance that this saturation itself would not suffice to render the body combustible,—and, to demonstrate this assertion, he gives the result of several experiments, in which he in vain tried to render ox-flesh inflammable by steeping it for several months in brandy, and even in alcohol and ether.

Another explanation has been proposed. Dr. Marc, and with him several other physicians, from the development of hydrogen gas which takes place in greater or less quantity in the intestines, have been led to imagine that a similar development may take place in other parts of the body, and that the gas might take fire on the approach of a burning body, or by an electrical action produced by the electric fluid, which might be developed in the individuals thus burnt. According to this theory, MM. Lecat, Kopp, and Marc, suppose, in subjects affected by spontaneous combustion, 1. An idio-electric state; 2. The development of hydrogen gas; 3. Its accumulation in the cellular tissue.

This latter explanation would appear to be confirmed by a very curious observation of M. Bailly's. That physician, on opening, in the presence of twenty pupils, a dead body, over the whole of which there was an emphysema, which was greater in the lower extremities than any where else, remarked, that, whenever a longitudinal incision was made, a gas escaped, which burned with a blue flame. The puncture of the abdomen yielded a stream of it more than six inches high. What was very remarkable, was, that the gases contained in the intestines, so far from increasing the flame, extinguished it.

M. Julia Fontanelle, for reasons similar to those which induced him to reject the first hypothesis, is of opinion that the presence of hydrogen gas cannot be admitted as the cause of spontaneous combustion. He founds this opinion more particularly upon experiments in which he in vain tried to render very thin slices of flesh combustible, by keeping them for three days immersed in pure hydrogen gas, in percarburetted hydrogen gas, and in oxygen gas.

Lastly, he considers the opinion equally untenable, that spontaneous combustion of the human body is owing to a combination of animal matter with the oxygen of the air, whatever may be the alterations which this matter may undergo: 1. Because a sufficient temperature is not developed; 2. Because, admitting this combustion as real, the residuum would be a charcoal, which could only be incinerated at a red heat, while, on the contrary, there is nothing but ashes; 3. Because one of the products of spontaneous combustion of

the human body is an unctuous substance, which the combustion of animal substances never yields; 4. Because it scarcely yields any ammoniacal products, while such are always produced by animal combustion.

After thus rejecting all the hypotheses hitherto proposed, M. Julia Fontanelle concludes that this phenomenon is the result of an internal decomposition, and is altogether independent of the influence of external agents. We give his own words:

"We consider," says he, "what are called spontaneous combustions of the human body, not as true combustions, but as intimate and spontaneous reactions, which depend upon new products originating from a degeneration of the muscles, tendons, viscera, &c. These products, on uniting, present the same phenomena as combustion, without losing any of the influence of external agents, whether by admitting the effect of the opposite electricities of Berzelius, or by adducing in example the inflammation of hydrogen, by its contact with chlorine, arsenic, or pulverized antimony, projected into this latter gas," &c.

It may be objected, however, that whatever may be the cause which induces this combustion, the caloric disengaged ought to be considerable, and consequently should ignite all the objects in the neighbourhood. We reply to this, that all combustible substances do not by any means disengage an equal quantity of caloric by combustion. Davy has shown, that a metallic gauze, having 160 holes in the square inch, and made of wire one-sixtieth of an inch in diameter, is penetrated at the ordinary temperature by the flame of hydrogen gas, while it is impermeable to that of alcohol, unless the gauze be very much heated. According to the same chemist, gauze of this kind, raised to a red heat, allows the flame of hydrogen gas to pass through it, without being permeable to percarburetted hydrogen gas. It is probable from this, that the products arising from the degeneration of the body may be very combustible, without, however, disengaging as much caloric as the other combustible bodies known, and without leaving a residuum as the two latter gases; and, in fine, we are of opinion, that, in some subjects, and chiefly in women, there exists a particular diathesis, which, conjoined with the asthenia occasioned by age, a life of little activity, and the abuse of spirituous liquors, may give rise to a spontaneous combustion. But we are far from considering as the material cause of this combustion, either alcohol, or hydrogen, or a superabundance of fat. If alcohol plays a prominent part in this combustion, it is by contributing to its production; that is to say, it produces, along with the other causes mentioned, the degeneration of which we have spoken, which gives rise to new products of a highly combustible nature, the reaction of which determines the combustion of the body.

It is to be regretted that the observations hitherto published are not more complete. We propose to ourselves to collect all that

may tend to throw light upon a subject so important in anthropology and medical jurisprudence.

From the *Lancet*.

CLINICAL LECTURE ON THE EXTIRPATION OF THE NECK OF THE UTERUS. By M. LISFRANC, Paris.

To the Editor of the Lancet.

SIR,—The following are notes which I took at a clinical lecture given by M. Lisfranc, previously to amputating the neck of the uterus of a young woman, in whom schirrus had commenced at that part. I am aware that you have already published, in your 12th vol., an account of this operation by Mr. Turley of Birmingham; but as there are some points mentioned in this lecture concerning the anatomy of the parts, and the symptoms following the operation, which he has not noticed, as well as a fuller description of the after treatment employed, you will, perhaps, not think it unworthy a place in your Journal; more especially, as this operation has not yet met with the attention which I think it deserves in this country.

I remain, Sir,

Yours obediently,

O.

M. Lisfranc began by giving a short history of this operation. The first attempt was the removal of the whole uterus through the vagina; this was successfully performed, in one case, by a surgeon in Switzerland. Langenbeck removed the uterus, in fifteen cases, by an incision above the pubes, as for the high stone operation; but all his patients died. M. Dupuytren removed the neck of the uterus by paring it *in situ* with a knife, curved laterally, having first introduced a speculum to guard the vagina and fix the uterus: this mode M. Lisfranc reprobated: the blood prevents you seeing whether you have removed all the disease or not; and the vagina may be wounded from the uterus receding before the knife. Many of his cases terminated fatally.

M. Lisfranc then proceeded to describe his own operation. This is to be commenced by introducing the speculum, (described by Mr. Turley, but for which Weiss's might be substituted with advantage,) the blades of which are then to be gradually separated; this stretches the upper part of the vagina, and brings the mouth of the uterus within the tube; the inner surface of which being polished, reflects the rays of a candle held at the orifice, and enables one thoroughly to examine the state of the part. The neck of the uterus should now be seized transversely with a pair of forceps, each blade of which terminates in two pointed curves, care being taken to apply them at an equal distance from the fore and back part, otherwise the uterus will descend obliquely, and the incision will be oblique, instead of perpendicular to the axis of the organ; in applying the forceps we should seize the

part rather suddenly, otherwise the instrument may slip and wound the vagina; we now remove the speculum, and gradually draw the uterus as low as we can; this point differs in different women, in some we can make the neck project some lines beyond the vagina, in others it will not quite reach the orifice; the latter case rendering the operation rather more difficult. An assistant should now separate the labia moderately, and another pair of forceps are to be applied perpendicularly to the former, with the blades above and below; for if we have only one pair, when the cut has past the level at which they are applied, the uterus is drawn obliquely by the part yet undivided; and instead of the incision being in one plane, a flap is formed, of some lines in length, on the fore part; the second pair obviates this inconvenience. The assistant now raises the neck of the uterus a little, to allow room for the knife; this is a curved blunt-pointed bistoury, the blade of which is sheathed with linen from the handle to within an inch and a half of the point; with it the neck is to be cut directly across from behind, at the requisite height; the forceps are then removed, and the uterus is allowed to retire.

The bleeding is not generally great; in only one of 22 cases, in which I have operated, have means been used to check it, and in this they were probably unnecessary, as they were employed by a young student in whose charge the patient had been left.

About an hour after the operation, vomiting generally occurs, returning every quarter of an hour for four, six, or even ten hours; this is, at first, alarming, the patient becoming pale and exhausted; the vomiting too generally causes a return of bleeding; but I am loth to use means to arrest it, as it discharges the uterus and prevents the chance of metritis. These symptoms subside gradually, and by the following day the patient appears quite well, the lancinating pains of the cause having disappeared, &c. In only one case metritis appeared, but readily yielded to a large application of leeches.

On the third or fourth day, the surgeon should introduce his finger into the vagina, and remove any clots which may have lodged there, as their putrefaction would irritate the wound. I think this better than using injections, which should not be employed until the seventh or eighth day, as the parts are as yet tender from the tractor, and there is danger lest the pipe be incautiously thrust against the wound. About the eighth day we may examine the part with the speculum, and we shall generally find it covered with healthy granulations and half cicatrized; we may now begin to use an injection of the solution of chloro-oxide of sodium, weak at first, gradually increasing the strength; the wound heals rapidly, but should a small ring remain sore, round the orifice of the uterus, we may employ the nitrate of silver in solution. In five or six weeks the patient is well.

In some patients, during the healing, some alarming symptoms occurred, viz. lancinating

pains in the uterus; these, however, generally proved to be preludes to the appearance of the menses, and disappeared with them.

The objections which have been urged against this operation are, 1st. That the peritoneum descends so low, and the insection of the vagina on the uterus is so narrow, that there is danger of wounding the peritoneum in removing the diseased part. 2d. That in many cases the neck is so softened as not to afford sufficient hold for the forceps. To the first I reply, that before I performed this operation, I examined the parts in upwards of 50 subjects at la Pitié, and in all I found that the insection of the vagina instead of being linear, occupied a space of three or four lines all round, and that the distance from the commencement of the insection to the fold of the peritoneum was seven lines in front, and six at the back, thus obviating all danger of wounding the peritoneum; even were we to amputate at the insection of the mucous membrane. To the second objection, I can only say, that where the neck of the uterus is so softened, the body will be always diseased, and that nothing but the complete removal of the organ can succeed.

The patient, a young woman, aged 24, was now brought in and operated on; the chief part of the pain was caused in drawing down the uterus; the incision also gave more pain than in either of the former cases I saw, (those described by Mr. Turley,) for then neither patient was aware when the incision was made, and thought, when the operation was concluded, that the preliminaries only had been made.

Bleeding continued for about four hours, about 14 oz. of blood were lost, which caused fainting. In the evening slight pain in the belly, for which she was bled to 4 oz., was rather sick once, but did not vomit. Passed a bad night; probably kept awake by a woman in labour in the same ward. On the following day she had a slight attack of gout in the middle finger of the right hand; she had had one attack previously.

The case continued to do well, and the wound healed kindly.

From the London Medical and Physical Journal.

RHEUMATISMUS FEBRILIS.

Paper on a singular Description of Disease which appeared in the Island of St. Christopher, in the latter end of the year 1827 and beginning of 1828. By JOHN SQUAER, Esq. Assistant Surgeon 93d Regiment.

The disease which forms the subject of the present paper had broken out for several weeks in the islands in our immediate neighbourhood to leeward, which are all foreign and "free ports," with the exception of the small island of Tortola, (which is a dependency of St. Kitt's,) and have frequent intercourse with us, by means of small English trading vessels;

consequently it is not to be wondered at, the facility with which disease of an epidemic nature finds its way here.

It is not meant that the present disease should be considered as having been brought to this island by means of communication: proof enough will be afforded to upset this idea in speaking of the comparative prevalence of this disease amongst the troops of the garrison and inhabitants, even although it is characterized in the newspapers of the island by the title of "*an epidemic of the most painful nature, which the oldest inhabitant did not remember to have seen or heard of before.*"

This disease is classed as a species of rheumatism, in the quarterly return of sick of this garrison, and is styled by me rheumatismus febrilis; commonly known by the name of rheumatic fever in this and the neighbouring islands.

In the latter end of December and beginning of January of the present year, many people began to complain of very violent head-ach, severe pain in the temples, shooting towards the forehead; frequently it was situated in the back of the head, stretching towards the neck and shoulders, which was one of its most painful positions, as the least motion created great agony, and it was difficult to find any easy posture for the head. There frequently was a painful sensation as if the head were drawn down towards one side or another; pain, or, at all events, a disagreeable sense of stiffness, was felt in the eyes, especially when moving them from side to side, or raising them upwards: the patients expressed it, by saying the socket felt as if it were too small for the eyeball; frequently the eyes felt painful to the touch; the adnata was slightly tinged with red vessels.

Shooting pains were at the same time felt in the back, loins, and thighs, particularly immediately over the knees, which soon became fixed and uncommonly severe: the same thing took place in the arms, forearms, wrists, fingers, knees, ankles, and feet, causing lameness; the calves of the legs were similarly affected.

A roseolar eruption came out early in the disease, which covered the wrists and extended up the forearm: it spread over the backs of the hands; the ankles and feet were in the same state; it was sometimes elevated in large wheals, and, when it affected the neck, it was extremely painful: the hands and feet were considerably swelled.

In delicate females, the roseola came out on the face in patches, and on different parts of the body, and remained for several weeks after the other symptoms had disappeared.

It need hardly be added, that motion of any kind greatly aggravated the symptoms, and the gentle pressure of the hand could scarcely be suffered.*

* The stiffened form, occasioned by the pains in the head connected with the shoulder, and the dread of motion, obtained for it the fantastic name of "the dandy."

Fever came on simultaneously with these various affections, or very soon was observed in conjunction with them, marked by a sense of heaviness in the head and great listlessness, nausea and loss of appetite, and, in delicate people, the irritability of stomach was sometimes distressing. Severe rigours, and alternating flushes of heat; face flushed; quick full pulse; and hot, dry skin; with, in a few cases, delirium, were also observed.

Pain of stomach, sensible to gentle pressure, was present in one or two instances.

The violence of the symptoms and fever lasted from four to five days; but it was never under seven or eight days that all the pains were gone. In most cases, the pains were felt for a much longer time; and in severe attacks, pain and tenderness to the touch remained in the eyes, hands, calves of the legs, ankles, and feet, for several weeks afterwards.

These symptoms varied in number and degree of violence, according to circumstances, and were much influenced by mode of living and constitution, sex, and age.

The soldiers composing the garrison of Brimstone Hill were less liable to this disease than the inhabitants; and their attacks were not of so long continuance, nor generally so severe. Nearly all the officers had it, and it was severe in one instance only. The very general run it took amongst the inhabitants had the effect of its being supposed to be epidemic; in many instances, not leaving a family till every one had been attacked.

The young and robust had smart attacks, and fever of shorter duration, and they did not so often labour under its effects; and were even exempt from one or two symptoms that afflicted people of an opposite description.

Delicate females and aged persons had more protracted attacks, and they suffered more from irritability of stomach; and the roseolar eruption in them was most remarkable; and the feet continued swelled and tender, producing lameness for some time: the fingers were also swelled and painful.

On account of the lingering nature of the disease, many were induced to suppose that, during the space of eight or ten weeks, they had fresh attacks, and were even impressed with the idea that they must have a third attack before they could get well: this was owing to exposure to the cold damp weather, which at first caused it, and consequently easily re-excited the pains they had not entirely got quit of.

This disease, in all the instances I have witnessed, was considered of a simple, and though of a violent nature, yet there was nothing dangerous in it. It has been said to have terminated fatally in one or two instances in this island: in some of the others; it has caused death in several instances.

This circumstance I am inclined to attribute to some untoward combination of disease, or might be the result of accident, as was the case in one instance. A coloured man, of the town of Old Road, having had symptoms of the disease, thought himself sufficiently well

even to go to his work, imprudently bathed in the river, which aggravated the disease to such a degree as to cause his death: previous to which, the irritability of stomach was very great, vomiting quantities of black-looking matter repeatedly.

Inflammation of the stomach, I am inclined to think, is the unfortunate combination which, in fatal cases, commonly is the cause of death. In a very few instances, I have observed it in the commencement of the attack, and it was necessary to direct particular attention to this symptom, or combination; for, as there is a possibility of this combination appearing in greater or less degree, so as perhaps to be little heeded, and be allowed to proceed too far, without any precaution being taken to remove it, it is at once accounted for how it may become the cause of death, and confirms the truth of this opinion.

Instances of relapses were few amongst the troops, and none of the lingering symptoms attached to them that have been enumerated in the description of the disease.

Children seemed for some time to be exempt from this disease, but latterly they have also suffered. It was indicated by peevishness, and soreness on being touched; great irritability of stomach; in those who were able to walk, an imperfect manner of using the limbs was observed, causing them frequently to fall. Feverishness was present in all.

There were a few peculiarities noticed in this disease, which entitled it to be considered as a novel and unknown kind of morbid affection: 1st, the extreme violence of the pains in the commencement, and the peculiar sensations they created; 2d, perspiration was not easily excited; 3d, thirst was not much complained of, even in the violence of the fever and in delicate females; 4th, the roseolar eruption above mentioned, and the swelling and tenderness of the hands and feet, was not often observed in the cases in the garrison; 5th, delirium was chiefly confined to delicate females, and aged persons of weak, nervous constitution.

The weather, previous to the appearance of the disease under consideration, and during its continuance, was of a nature unprecedented in severity in the West Indies, at least for very many years.

In the latter end of November, and nearly up to the present period, the weather became extremely boisterous, being nothing but a continuance of heavy rains and high winds; the evenings cold—very cold for this country, so much so that we were obliged to shut our doors and windows on sitting down to dinner; and we found it requisite to cover ourselves with a blanket at night. No Creole constitution could hold out against such weather: they are generally of such a frame that they are not at all capable of undergoing the fatigues and exposure of Europeans who have been a few years in this country. Besides, their mode of living and their habits are also very different from the regiments serving in the country. It was even surprising we escaped as we did;

and, although the dress of our men was not altered, which at the time was merely linen trowsers, and which could not guard them very well from the cold damp air of the night, yet we did not suffer in any thing like the way that the natives did. The greater part of our men are young, of strong constitutions, and well fed: it was not to be supposed that they could easily be affected by weather that to them must have been only agreeable.

In treating this anomalous disease, the objects had in view were to lower increased action in the system, and to restore the deranged functions of the vessels of the skin, which might be almost considered the cause of the disease. In accomplishing these purposes, few means were required. The pain of head, and great degree of excitement in young men of stout habits, sometimes required blood-letting, but, generally speaking, it was seldom employed: there was a much better remedy found, which nearly answered both purposes, and that was cathartics. Aloes, colocynth with a combination of calomel, in the following proportions and manner of exhibition, was the usual plan adopted:—*R. Extract. colocynthidis comp., aloes socotrinae, gummi resinæ, aa gr. iij.; hydrargyri submuriatis gr. xxiv. fiat massa in pilulas duodecim dividend sumantur tres h. s.* On the following morning, the pills were assisted by doses of infusion of senna, to which was added a small quantity of neutral salts, or supertartrate of potass. This plan generally required to be repeated once or twice.

When the action of the bowels was thus increased and kept up, the febrile action and violence of the symptoms underwent an almost immediate diminution; particularly the headache, which was the most distressing symptom. Cold water was at the same time applied to the head, by means of folds of linen.

To determine to the skin was another mode of treatment employed, to remove altogether the pains of different parts of the body: this was effected by using the warm bath, and giving small doses of antimonial or James's powder, with a few grains of calomel, three or four times a day; and keeping the body warm, from the commencement of the treatment.

The diet was light and plain. Wine, when it might be advantageously employed, was given.

When pain of stomach was present, which very rarely was the case, and, when it was increased by pressure, its removal was always of the first moment; in doing which, the counter-irritation occasioned by the application of vesicants was a very powerful remedy. Gentle purgative injections at the same time were essentially useful.

With regard to the employment of the sulphate of quinine, I am not able to bear testimony of any power it is supposed to possess in diminishing the violence of the symptoms, or in preventing returns of this disease; and seeing no reason to believe that there existed any morbid consent between the sensorium and deranged impressions of distant parts, I never

employed this medicine with the view of defeating its return.—I must confess I used it (the sulphate of quinine) in one or two instances only, before the nature of the disease was exactly declared. It came on in subjects accustomed to frequent attacks of ague, the symptoms of which were chiefly complained of at the beginning. It afterwards turned out that violent pains, such as those that characterize rheumatismus febrilis, were conjoined; therefore cannot positively decide in favor of the sulphate of quinine having any effect in shortening the disease, or in preventing its recurrence when it had apparently gone off.

The disease was very apt to return, or, from having disappeared, was liable to be again excited, if the patients were unguardedly exposed to its causes, which have been stated to be an extraordinary degree of cold and damp in the atmosphere, and the prevalence of high winds, with heavy rain. Under these circumstances, the best security that could be had against its aggravation or recurrence was to defend the body by warm clothing, and confinement to the house, or even to the bed-room.

During the last six weeks we have had rather better weather, but it is still far from being settled, and of late the disease has not appeared in any thing like the frequency it did some time ago.

St. Christopher's, April 11th, 1828.

From the London Medical Repository and Review.

ON THE LIVER, AS AN AUXILIARY PULMONARY ORGAN, with some Practical Remarks on the Effects induced by its Derangement.

Whether with respect to human labours the "quid utile" can be always beneficially answered, it is difficult to say: but with regard to the works of the Creator, the response must always be in the affirmative. Every thing which He has created has its use; and although, in reference to many parts of the human system, we are as yet unable to point out their uses, yet that these parts have uses, no intelligent mind can doubt. In proportion as anatomy, physiology, and pathology are pursued, uses are found in addition to those which were previously supposed, and even imagined uses are discovered not to be real. There is often an unfortunate haste in ascribing wisdom to the Creator, where He has not shown any, at least in the way in which the wisdom is considered as being shown. This fact shall be kept in view in the following remarks: and a due modesty will be preserved in speaking of the uses of the important viscus, the liver, now under our consideration.

There is no need to enter into any anatomical detail respecting the liver generally; and any description that is to be given will be limited to those peculiarities that seem to favour the view pointed out above, that the liver is an auxiliary pulmonary organ.

The use commonly ascribed to the liver is

that of aiding digestion, by favouring the separation of the nutritious and excrementitious parts of the food, and by communicating a peculiar stimulus to the feculent part, thereby aiding its expulsion. That this use is not sufficient, is an opinion founded upon several circumstances. The first to be mentioned is the large size of the liver in the fœtus, and the quantity of meconium contained in the intestines. The size of the liver being so great in an individual where no process of digestion goes on, must be for some other purpose: and this, it seems justifiable to suppose, is the separation of a quantity of impure matter from the blood, which, if carried into the circulation, would be injurious. Again, in jaundiced people digestion goes on: and Dr. Fordyce, having tied the ductus communis choledochus, found that the formation of chyle took place as before.

The peculiar situation of the liver with respect to the circulation of the blood, seems to favour the view that it is an auxiliary pulmonary organ. The blood, rendered impure by the different changes going on in the system, is about to be returned to the lungs. Loaded with impurities, it is brought to the liver on its way, by veins, and from this impure venous blood, the fluid called *bile* is secreted. Why should the liver, were it not for some purpose of this nature, differ from all other viscera, in having, as its fluid on which to act, *venous*, and not *arterial*, blood? And why should the liver be placed, as it were, just at the termination of the venous circulation? And why should the blood of the intestines, which, it seems reasonable to suppose, must be the most impure part, be that which, in particular, is made to pass through the liver? All these questions seem difficult to answer, unless we allow that the liver is an auxiliary pulmonary organ, acting as such, by *separating from the blood, principles, which, if carried into the system, would be highly injurious.*

In further illustration of this view, some facts, showing certain similarities between the vessels of the lungs and of the liver, may be mentioned.

The pulmonary artery, like all arteries, has the power of contraction; and Magendie states, that the branches of the vena portæ are the only ones which, by the disposition of their external membrane, are able to contract when the blood which passes through them is diminished in quantity. The pulmonary veins, too, as contrasted with the venæ cavæ hepaticæ, have no valves, and do not anastomose after they have acquired a certain thickness. In both these respects the two sets of vessels agree. Barclay says, "In all the arteries, the blood flows from the trunk to the branches; in the veins, it flows from the branches to the trunk. The vena portæ is the only exception. This singular vein is ramified at both extremities. By the branches of one extremity it collects the blood from the stomach, spleen, pancreas, and intestines; and by the branches of the opposite extremity distributes it through the liver."

From the various anomalous circumstances with respect to the pulmonary artery and the vena portæ, it seems difficult to say whether the pulmonary artery resembles a vein more than the vena portæ resembles an artery. This difficulty seems to have presented itself to ancient anatomists, for "prior to the time of Harvey, all the blood-vessels connected with the right or pulmonic ventricle were considered as veins, and hence, in those days, the pulmonary artery was called the *vena arteriosa*, or *arteria venosa*. In the same way all the vessels connected with the left or systemic ventricle were considered as arteries; and hence the pulmonary or systemic veins were regularly termed *arteriæ venosæ*, or *venæ arteriosæ*."—Barclay on the Arteries.

Some morbid manifestations, which seem to favour the view that the liver is an auxiliary pulmonary organ, may now be stated, being premised by a brief statement of the chemical composition of the bile, and also of the change which the blood undergoes in the lungs.

The *proximate* elements of bile seem to be water, resin, picromel, albumen, soda and its salts; perhaps a little phosphate of lime, and some oxide of iron. These, reduced to their *ultimate* elements, will give a great quantity of *carbon*, besides *hydrogen* and *nitrogen*. Thus, it will be seen, that by the liver a considerable quantity of carbon is separated from the blood. Again, with respect to the change of blood in the lungs, every one knows that it consists, in a great measure, in the evolution of *carbonic acid*. Here then we perceive a similarity in the function of the liver and of the lungs: in both the separation of carbon is the consequence of their action. This short statement may be concluded by a passage from Magendie: "On account of the considerable extent of the mucous surface, with which the drinks or other liquids are in contact, and of the rapidity of their absorption by the mesenteric veins, a considerable quantity of liquid, foreign to the economy, traverses the abdominal venous system in a given time, and changes the composition of the blood. If this liquid arrived at the lungs in this condition, and proceeded thence to all the organs, very serious inconveniences might arise."

Now as to the morbid manifestations. Every one who practises in a great city must have met with cases similar to the following:—

J. A., aged 26, troubled with a swelling of his right side, complains of a weight there, and of pain upon pressure; his bowels constipated, requiring, generally, medicine to keep them open; has frequently, almost constantly, an oppression of spirits, with a sense of tightness across his chest, making him every now and then take a full breath for relief; great drowsiness; horrible dreams at night; and is so little refreshed by sleep, that he finds great difficulty in rising in the morning; and when he does rise, is very little refreshed: the tongue in the morning is covered by a thick fur. He experiences a general languor, and is better when any powerful motive calls him into bodily and mental activity, and also when in the country.

He has a peculiar pulsation at the pit of the stomach, detected by the stethoscope. He looks well; so that his friends imagine he is not so unwell as he pretends to be: eats moderately, and drinks freely.

Such cases, I imagine, are very common. They come across my path, and are often treated with success by using means which have the effect of rendering the circulation through the liver perfect, and thereby restoring the secretion of bile, which, being in proper quantity, stimulates the intestines, and enables them to act afterwards of themselves.

These symptoms seem to arise from the bile not being separated in proper proportion from the blood, which, thus impure, passes through the lungs, and from its state, oppresses that organ, and is carried to the head, inducing that heaviness, that unwillingness to think or act, that sluggishness, those unpleasant dreams. The bile being deficient, the intestines want their appropriate stimulus, and hence cannot act. The peculiar pulsation indicates an impediment to the flow of blood through the liver, which aids the effect of the impure blood in producing the symptoms already described.

The authority for the correctness of these views is founded upon a series of observations, but more particularly upon some humoral pathological facts which have come under my own notice, and some stated in a work, by Dr. Foote, on cholera Indica. To these the reader's attention is particularly requested.

In this disease there is no secretion of bile, at least of bile properly formed; it thus differs from the common cholera in this country. The patient complains of weight in the epigastrium and hypochondria; complains of a tension about the abdomen; exhibits the greatest anxiety; is unwilling to be disturbed. The disease is generally preceded by vomiting and purging; but in those cases, where these symptoms do not precede, the collapse is instant, and the individual dies in a soporific state. There is a peculiar pulsation at the pit of the stomach. On dissection, the liver is found gorged with blood, the veins filled with blood of a blackish colour, and the intestines filled with fetid air. The gall-bladder is frequently found full of thick bile, resembling pitch. The bladder is constantly empty; the lungs exceedingly collapsed; the veins of the dura mater are turgid, and, according to Finlayson, the brain seems covered with general ecchymosis. The blood, too, (which is a curious circumstance,) when a vein is opened, during life, flows very tardily; sometimes, indeed generally, it dribbles away, drop by drop, and is blackish.

Dr. Foote, from all these facts, concludes that there is an *excess of carbon and nitrogen in the blood*, the secretion of the bile and of the urine, and that from the skin, being stopped; and that the excess of these deleterious ingredients, acting either *per se*, or producing some deleterious ingredient by a union together, induces the symptoms described.

Dr. Foote also states a very interesting fact, namely, that the tension experienced in cholera Indica, and the non-coagulation of the

blood, are symptoms occurring from the bite of the snake called *Cobra di Capello*; that the peculiar fetid air found in the intestines, is met with in individuals bitten by poisonous fish; and that the above state of the blood occurs in asphyxia, in those killed by the inhalation of carbonic acid, by lightning, by malignant fever, and by the plague.

These are very curious facts, and they testify, with the foregoing, the effect produced when the liver does not perform its function. Indeed, the symptoms of cholera Indica bear a very near relation, the difference being in degree, to those which have been described in the case of J. A.; and from long observation, the results of which may, perhaps, hereafter be more fully detailed, it is concluded that the symptoms of the case of J. A. were produced by the liver not performing its function as an auxiliary pulmonary organ, of separating from the blood those principles, carbon and nitrogen, which remaining, are so deleterious. And this view is strengthened by the fact, that fresh country air is often very successful in relieving the torpidity of body and the listlessness of mind occurring in such morbid states. This subject I at present leave. I appear to some, no doubt, to tread very closely upon the humoral pathology; but this I regard not—truth is my aim, and to do good is my object; names alone I hold to be trivial things; they should never, to a philosophic mind, be an object of terror.

Some affections of the lungs, induced by a diseased state, may be noticed at some future opportunity.

M. D.

London, June 15th, 1828.

From the Medico-Chirurgical Review.

THE INFLUENCE OF ANATOMICAL VARIETIES ON SURGICAL OPERATIONS. By M. ROBERT.

Anatomical deviations have been studied physiologically, and from this study have been deduced the laws of their formation, and the development of monstrosities. The study of these varieties of structure, under a surgical point of view, would be of great benefit to the surgical word, and to society at large. Suppose, for example, a surgeon is called on to operate for an aneurism of the brachial artery at the bend of the arm, and is ignorant that this vessel may bifurcate as high up as the axilla. Instead of searching carefully for the vessel which is diseased, he ties that one which lies in the usual place. Meanwhile the tumour enlarges, and at length bursts. Attention to the study of anatomical varieties prevents such oversights as these. It is of great importance that the surgeon, when operating, should be acquainted with all the anomalies of structure which the part may possibly present. With this knowledge in his mind, he will not be alarmed or embarrassed in his operation, should the anomaly exist. M. Robert has evinced great industry and research in this extended essay, and the

order which he follows in his inquiries, consists in a successive examination of the great regions of the trunk and the extremities, subdividing his researches into varieties of structure in the bones, the ligaments, the muscles, the vessels, the nerves—and lastly the viscera themselves. We shall follow our author, and endeavour to present our readers with a succinct account of his researches.

I. THE HEAD-BONES, AND FACE.

The cranium presents great varieties in structure and shape. We often meet with depressions and protuberances, which might be confounded with sanguineous tumours or fractures of bone, after contusions. The defect of ossification, in consequence of which the fontanelles sometimes remain unclosed, is a common variety—but it is not so well known that the brain sometimes protrudes through these apertures, and forms an *encephalocoele*, which becomes strangulated as the bones thicken; and requires the trephine at the sides of the unnatural opening. In very old people, the skull becomes so much attenuated, in some places, as to render operations hazardous, if this circumstance be not borne in mind. The frontal bone has remained divided by a prolongation of the sagittal suture, and has been taken for a fracture, of which a remarkable instance is recorded by Quesnay.

A very curious case of development of the vessels of the diploe is given by Frank.

A peasant, aged 24 years, in a scuffle with a soldier, received a sabre wound on the anterior and right side of the frontal bone, an inch and a half from the sagittal suture, where it is obliterated at this place in the adult. The sabre had removed a portion of the external table of the skull. The man neglected himself for some days, and when Frank arrived there were such symptoms as induced him to apply the trephine in the neighbourhood of the wound. As soon as the saw had arrived at the diploe, a torrent of blood issued forth—and, in short, the patient died of the hæmorrhage the same day. On dissection, it was found that there were seven vascular communications between the dura mater and the diploe, through so many foramina in the internal table of the skull, at this place. Hence the fatal hæmorrhage. More recently, Magendie attributes to this kind of hæmorrhage, the majority of those cases reported by writers, where *extravasated* blood has issued forth after the application of the trephine.

A great many varieties occur in the anatomy of the face. The supra-orbital foramen may be placed quite behind the orbit, making it extremely difficult to divide the frontal branch of the fifth pair, in the *tic douloureux*. The *os unguis* is sometimes wanting, and its place supplied by the nasal process of the superior maxillary bone. In such a case considerable difficulty would be experienced in perforating the bone for fistula lachrymalis. In some individuals a middle bone is found between the two portions of the inferior maxillary; it may either be single or divided by the

symphysis into two, and, from its mobility, has been mistaken for a fracture of the lower jaw and treated accordingly. There may be two ranine arteries, so that in glossitis our author prefers making deep incisions on the dorsum of the tongue, to opening the ranine veins. When, as sometimes happens, the *membrana pupillaris* remains after birth, it is necessary either simply to puncture it, make a free division, or cut away a point of the circumference of the iris, and form an artificial pupil. The inferior orifice of the lachrymal canal is frequently guarded by a valve, obstructing the passage of the stilet. There are sometimes two stemonian ducts which should be remembered in the operation for salivary fistula. Not unfrequently the spinous process of the seventh cervical vertebra, is separate from the rest of the bone and moveable beneath the skin. After an injury upon the part, this might readily be mistaken for a fracture. The *scalenus medius* of Soemmering, when it exists, separates into two bundles the nerves of the axillary plexus. This would cause much embarrassment in the operation for tying the subclavian. Sometimes the muscle is directly interposed between the artery and nerves, which would completely protect the latter; and in a child, which we had an opportunity of examining last summer, there was no *scalenus anticus* muscle at all, so that the subclavian vein lay in contact with the subclavian artery. The *omo-hyoideus* may arise not from the scapula, but from the middle of the clavicle, which would also embarrass the operator on the subclavian.

II. VESSELS OF THE NECK.

Allan Burns mentions an instance, and Harrison, we believe, gives another, where the *right* subclavian arose from the *descending* arch of the aorta, and passed across the neck in front of the trachea, as high as the inferior border of the thyroid gland. In such a patient, were tracheotomy performed, the artery would most inevitably be wounded. It must have been some irregularity in the distribution of the great vessels which happened to a celebrated professor of surgery at Berlin, who having performed the operation of bronchotomy on the daughter of a brother-professor, the patient died under his hands.* The subclavian may take the same origin but pass behind, instead of before the trachea, between it and the œsophagus. In such a case, care would be required in performing œsophagotomy. A third variety in the course of the right subclavian, is where it takes its rise from the arch of the aorta, beyond the origin of the left subclavian, and on the left side of the body of the second dorsal vertebra. It then crosses in front of the spine, and behind the thoracic duct and œsophagus, lying much exposed to wounds from foreign bodies sticking in that tube. In 1815, Mr. Kirby† was summoned to

* Walter. Mémoires de l'Académie de Berlin.

† Dublin Hosp. Reports, v. iii. p. 324.

a woman, who had swallowed a piece of bone, and who, in spite of tracheotomy, died of suffocation and hæmorrhage, apparently from the interior of the œsophagus. On dissection, it was found that a spicula of bone had pierced the posterior wall of the œsophagus, and transfixed the right subclavian, which took this unusual course. When there is this variety, the nervus vagus on the right side, occasionally sends off no recurrent branch, its place being supplied by several branches arising from the inside of the vagal trunk. In such a case, if the carotid were tied these branches would probably be cut, and the voice perceptibly affected. Immediately after its exit from between the scaleni, the subclavian, in some instances, becomes fairly enveloped in the brachial plexus. Perhaps it was owing to this anomaly that the nerve was tied instead of the subclavian, in the case related by Sir Astley Cooper. Zagorsky has observed, that the innominate on the right side was entirely wanting, but it existed on the left.*

It has been well remarked by Hodgson, that aneurism of the innominate, or even of the arch of the aorta, will frequently rise so high in the neck as to be mistaken for aneurism of the carotid, or even of the subclavian. A case of the latter kind is mentioned by Allan Burns, where it was proposed to tie the subclavian artery. Fortunately, however, this operation was not performed.

The right carotid may arise from the arch of the aorta, instead of the innominate. It then mounts up a couple of inches, or more, in front of the sternum, and crosses the trachea, lying exposed, of course, to injury in tracheotomy and wounds of the neck. Both carotids, on the other hand, may arise from the innominate, and then the left takes the course in front of the trachea. In such a case it may admit of doubt whether a ligature of the innominate would not be attended with very serious results in consequence of the stoppage of the flow of blood at the same instant through one subclavian and both carotids. Portal has related a case in which extirpation of an enlarged tonsil proved fatal, in consequence, it would seem, of a wound of the internal carotid, which lay extremely near it. Barclay, also, mentions a similar case.

Allan Burns observed in a child, that the left vertebral artery took its rise from the arch of the aorta, whilst the right arose from the subclavian, and passed up behind the carotid, along with the sympathetic nerve, as high as the third cervical vertebra, where it entered the osseous canal in the transverse processes. In such a case, there would be some danger of including this artery in the ligature if it were necessary to tie the carotid, and besides, an aneurism of the artery in this situation would

be readily confounded with one of the carotid itself. However, if a surgeon follows the excellent rule of never drawing the knot upon a vessel till he is satisfied that pressure of it between his finger and thumb stops the flow of blood into the sac, this blunder can never happen. Some varieties occur in the distribution of the thyroid arteries which we shall briefly notice. The superior may arise from the common carotid low in the neck, pass up alongside of that vessel, and then when it has arrived at or near the angle of the jaw turn downwards to the thyroid gland. In this variety the vessel is large, and would be in the way both of the external incisions and the ligature in the operation on the carotid trunk. The inferior thyroid also, having taken its usual origin, may pass directly in front of the common carotid below. Burns saw both inferior thyroids arise by a common trunk from the subclavian or aorta, which would add to the danger of œsophagotomy. In tracheotomy, it should be remembered, that there is very frequently indeed a large artery, called by Harrison the thyroideus medius, which arises from the common carotid, and passes in front of the trachea. Burns mentions a case where this artery arose from the innominate, and passed upon the œsophagus as high as the left lobe of the thyroid gland. The patient was a young lad, and œsophagotomy having been performed, the bleeding from the vessel was so considerable as to require a ligature.

In the operations on the neck, the veins are often sadly in the way of the surgeon's knife. There may be two or even three external jugulars on one or both sides. Another variety has been noticed by Wilde, where a vein as large as the jugular, was formed by the union of two branches in front of the trachea, and proceeding upwards from thence dipped down by the side of the thyroid gland to join the internal jugular. Of course in laryngotomy and tracheotomy, such a distribution might be productive of considerable embarrassment. A case is related by Virgili in the *Memoirs of the Academy of Surgery*, where, having opened the trachea between the rings, such hæmorrhage took place, that the blood getting into the trachea excited a most violent paroxysm of coughing, forcing out the canula whenever it was introduced. In this dilemma, the man being almost suffocated, Virgili cut longitudinally into the trachea down to its sixth ring, and held the patient's head out of bed, with his face towards the ground. This operation was perfectly successful. M. Roux, in a similar case, introduced a silver catheter into the trachea, and sucked out the blood.

The subclavian artery, external to the scalenus, where it is usually tied in operations, is described as lying in a triangular space, the base of which is formed by the subclavian vein, and the sides by the scaleni muscles, and axillary plexus. The vein, however, is sometimes placed much higher than it *should* be. A man had a large aneurism of the subclavian, and the operation for tying this vessel was per-

* The innominate has also been seen rising so high in the neck as to be endangered by the razor of the suicide, or the knife of the tracheotomist.

formed in the usual situation. The tumour was a good deal in the way, but the operator at last arrived at what he considered was the artery, and he was confirmed in this opinion, by finding that, on raising the vessel, the pulsation in the tumour ceased. The ligature was applied—bad symptoms supervened—and the patient died. On dissection, it was discovered that one of the brachial nerves and a *part* of the subclavian vein had been tied, the needle having passed *through* the latter vessel, which lay higher in the neck than usual. The cessation of pulsation in the sac, on lifting up the nerve, was explained, by its having dragged up the artery with it so as to produce considerable compression. The subclavian vein has likewise been seen passing *with* the artery, *between* the scaleni; in this variety it might either be tied for the artery, or both included in the one ligature. Morgagni found two subclavian veins, uniting where they joined the internal jugular, which would also be an awkward anomaly.

III. VARIETIES IN THE ANATOMY OF THE THORAX.

These are few. The last rib may be deficient, with or without deficiency of the corresponding vertebra. An intercostal space will then be wanting, which should be recollected in the operation for empyema. Two ribs may be joined before or behind, or they may be entirely joined together, and then the double rib is commonly inserted into the sternum by two cartilages. Many varieties occur in the location of the muscles, but these we shall pass over, as they are of little practical importance. There may be two intercostal arteries, one taking the course of the mamma interna, the other lying in the middle of the intercostal space, and exposed in the operation for empyema. In a phthisical patient opened by Morgagni, death was occasioned by rupture of the vena azygos, which was as large as the superior cava.

This concludes the first part of M. Robert's Memoir, and we freely award him our tribute of approbation for the patient industry in collecting facts which it evinces. Such men are the pioneers of science, and their task, though uninviting and laborious, is of infinite importance. The subject of surgical anatomy is scarcely paid sufficient attention to in this country, for if it were, we should have fewer opportunities of seeing reports of errors and unfortunate operations blazoned forth before the gaze of a credulous and greedy public. We have been at considerable pains to detail the varieties which occur in the vessels of the neck, as the anatomy of that truly *surgical* region is complicated at the best, and the blunders which have been made in it, even lately, are unluckily notorious. In a succeeding number of the *Journal des Progrés*, we are promised a continuation of the Memoir, and we shall take care to lay an abstract of it, when it comes, before our readers.

From the Medico-Chirurgical Review.

HOSPITAL REPORT OF M. CHOMEL, from La Charité, between the 1st of February and the 1st of September, 1827. Reported by M. DE LAGARDE.

1. PERIPNEUMONY.—The total number of cases [of all kinds] treated in the above period are 281, of whom 39 died. The proportion of acute diseases was greater than usual, as compared with the chronic. To show how uncertain is the ratio of mortality in the same disease at different periods, we may remark, that out of eighteen cases of peripneumony, occurring in the spring quarter of the above period, six died—while in an equal number of peripneumonies occurring in the summer, there was not a single death. The treatment adopted by M. Chomel (who is an excellent practitioner) was active depletion, adapted to the violence of the symptoms and strength of the patient. But although this treatment was put in force, even from the earliest dawn of the inflammation, the later could not always be checked; but pursued its course to a fatal termination. Large doses of antimony were tried in some cases, with success—in others, without good effects.

2. INTERMITTENTS.—Twenty-four patients labouring under intermittent fever were admitted during the half-year. The types were various, and some were complicated with slight inflammations, not requiring any active treatment. A few days were always allowed to pass before the febrifuge was administered. In eleven cases the cure was spontaneous, without any medicine, and merely from change of scene and proper diet. When the ague resisted this process, the quinine was given in doses of not less than six grains in the 24 hours, with complete success.

3. ANEURISM.—One case of aneurism of the arch of the aorta, with pulsating tumour below the left clavicle, presented itself. The patient was a washerwoman, aged 38 years. The plan of Valsalva was tried, but without success, and the patient left the hospital, tired out with the starvation and depletion which she underwent there, without any apparent benefit.

4. GASTRIC COMPLAINTS.—Several patients labouring under derangements of the stomach [embarras gastrique] were treated during the semestre. Many were cured by low diet, repose, and acidulated drink. In some cases it was necessary to prescribe emetics. In a certain proportion of cases, these were extremely serviceable, and removed the symptoms very quickly—in others, the emetics were injurious—"a fact which proves that stomach affections are not all of the same nature, and to be cured by a routine treatment."

5. ACUTE RHEUMATISM.—Most of these cases were managed by gentle antiphlogistics and low diet, requiring from two to four weeks for cure. In one case, which was very severe, and where almost all the joints were swelled and inflamed, the large doses of tartar-emetic were employed (six grains a day) without *much*, indeed with scarcely any good effect.

The **TOLERANCE** (a term applied to the period when the antimony ceases to cause sickness) was established on the second day; but little or no progress was made, the rheumatic inflammation rather shifting its seat than quitting its hold. After a month's sojourn in the hospital, the patient went away, by no means cured. It does not appear that our Gallic neighbours venture much on bark, arsenic, mercury, or other heroic remedy in this distressing disease.

6. **AMENORRŒA**.—The general practice in France is to apply fifteen or twenty leeches to the pudendum. M. Chomel prefers applying three or four daily, for five or six days, at the expected menstrual periods, in order to imitate more nearly the process of nature. Previously to the expected epoch, he orders cupping-glasses (dry) to the upper and inner part of the thighs, and warm vapour baths to the lower part of the body.

We shall now proceed to analyse some of the cases detailed in this report.

7. **PNEUMONIA CARDITIS**.—A plumber, aged 40 years, previously strong and healthy, with the exception of some catarrhal affections, felt a shiver, on the evening of the 6th February, 1827, followed by pain in his right side. He went to work the next day, but on the 3d he was obliged to take to his bed. On the 10th Feb. he entered the hospital. Below the right nipple there was acute pain, increased by deep inspiration, with inability to lie on that side. The cough was frequent, the expectoration free and mucous, the sound, on percussion, rather dull, even on the left side. In the right side of the chest the "râle sonore" and the "râle crepitant" were heard in several points. In the posterior portions of lung, in this side, the respiration was pretty natural. In the *left* side the respiration was every where heard. He had been bled the preceding evening, and the blood was highly inflamed. Fifteen leeches to the side affected. Ptisans. 11th. Little alteration. Venesection to $\text{ʒ} \text{ xij}$. was ordered, but not carried into effect. 12th. The symptoms are exasperated, and the sonorous wheeze is heard in the left side of the chest. Bled to twelve ounces. 13th. No diminution of the symptoms—blood not much inflamed. To take six grains of tartar-emetic in the next 24 hours, in an emulsion. 14th. The patient is more quiet—no alteration in the state of the chest, as indicated by auscultation—four stools from the antimony, but no sickness. Tongue natural—no tenderness at the epigastrium. The antimony to be repeated. 15th. The countenance more sunken. Has had two stools. The antimony to be increased to twelve grains. 16th. Very obscure in the region of the heart, on percussion—respiration very quick—cough and expectoration the same—no stools, no vomiting—pulse 108, and irregular—great debility. Tartar-emetic to be increased to 24 grains. 17th. Had abundant perspiration last night—scarcely any respiration to be heard in the front of the left side. Antimony increased to 36 grains.

We need not pursue the case any farther.

The patient died on the 20th of the same month.

Dissection. There was no diseased appearance in the head. The left lung was everywhere adherent, and the superior three-fourths of that lung were hepatized—the inferior portions not quite so, but approaching to that state. The right lung presented the different degrees of hepatization. There was some ulceration in the mucous membrane of the larynx, about the glottis. There was pericarditis, and considerable effusion into the cavity of the pericardium, of almost a purulent fluid. The whole of the heart was covered with false membranes from a line to two lines in thickness. There were several small ulcerations, and other marks of inflammation in the mucous membrane of the œsophagus, stomach, and intestines.

Reflections. M. Chomel evidently anticipates the remark that depletion was too sparingly employed at the commencement of this disease. He excuses himself by observing that the two general bleedings produced no mitigation of the symptoms. But what are two bleedings of twelve ounces each? Double the quantity would have been a great deal too little. Besides, the day that was allowed to elapse between the first and second bleeding, was a fatal neglect. *Bis dat qui dat cito.* But M. Chomel, or his reporter, tells us that, in the same ward, a few days previously, a man had been attacked with pneumonia, and was very vigorously depleted from the beginning, yet still the inflammation went on to a fatal issue. Hence he concludes that there was something specific or malignant in the peripneumonies of the spring of 1827. There might be so; but we question whether either of these cases presented any just grounds for not carrying depletion to a much greater extent, without attending to any thing but the disorganizing inflammation that was evidently advancing daily on a vital organ.

Whether the large quantities of tartarized antimony which were swallowed during the illness, without producing either vomiting or purging, had any thing to do with the ulceration and inflammation observed in the primæ viæ, we will not pretend to say, since much greater devastations are seen where no medicine of this kind had been administered. But, considering the effects which this substance produces on the external surface, we should not wonder if a corresponding degree of irritation might not be produced by such large exhibitions internally. We shall make room for one more case.

8. **MOLLESCENCE OF THE COATS OF THE STOMACH**.—A married woman, aged 22 years, who had had one child, became troubled with a considerable menorrhagia in the beginning of the year 1827, at which period she was also exposed to several moral emotions of a distressing nature. Nevertheless she became pregnant, and experienced almost constant malaise—anorexia—thirst—tenderness at the epigastrium after eating—and, finally, vomiting of yellow and bitter matters. It was three days

after the commencement of these more serious symptoms that she entered La Charité, viz. on the 24th May, 1827. The expression of the countenance was natural, as was the state of the skin and tongue—the pulse was scarcely quickened—thirst very moderate—epigastrium very tender on pressure, but the abdomen soft and indolent. Each day she had ten or twelve vomitings of bilious matters, with some streaks of blood—stools regular. Leeches were applied to the epigastrium, and fomentations, lavements, diluents, &c. were employed, but without success. The vomitings continued—the tenderness of the epigastrium increased—the tongue was sometimes red, or shining—sometimes natural. Opium, for a time, diminished the sickness, but ultimately failed. On the 24th June, the sickness suddenly ceased—and the epigastric pain vanished entirely. But debility and emaciation advanced, and she expired on the 9th July, no vomiting having occurred for a fortnight before dissolution.

Dissection.—On opening the abdomen, the stomach was found torn from the cardiac orifice to about the middle of its anterior surface; but without any extravasation into the abdomen. A great portion of the mucous membrane of this organ was completely destroyed, and some parts of the muscular and peritoneal coverings were so soft and thin as to be ruptured almost by handling them. There were only a few red patches in the mucous membrane of the intestines. The uterus contained a fœtus of three months.

Remarks.—This was an extremely well-marked case of gastritis, (of the mucous membrane,) as far as pathology was concerned. But it is not a little remarkable that, while such a dreadful disorganization was going forward in a vital viscus, there should be so little febrile disturbance in the system. The pulse and skin scarcely evinced any deviation from a state of health, and the tongue was often natural. The cessation of the vomiting too, for a fortnight before death, was an occurrence not to be expected, according to the ideas which are formed from elementary instruction, and systematic descriptions of diseases. It is from clinical experience, and from faithful clinical reports, that the mind becomes stored with the knowledge of those almost infinite varieties presented in diseases, the want of which knowledge renders the practitioner liable to perpetual error in prognosis and diagnosis. The apparently dry details of a case of this kind are quite wearisome, if not disgusting to the young, and especially to the routine practitioner. But we can tell them, that a careful perusal of such cases is one of the best modes of disciplining the mind for receiving accurate impressions at the bed-side of sickness. There is a very prevalent idea among professional men, that *practice alone* makes the good and successful practitioner. We deny it—and this denial is grounded on more than 30 years of careful observation, not only of disease, but of men. In all that course of time, we never knew a good and successful practitioner who

did not read and study, as well as observe. It is usual for the lazy man of experience to quote John Hunter, as an example of great eminence, without reading. Not having known John Hunter, we cannot speak as to his *practical* talents; but the foregoing opinion is the result of what we have seen among our own acquaintances, which are not very few. It is fashionable to deride books and study; but, for our own parts, we have no hesitation in affirming, that nine-tenths of our *practical* knowledge would never have been acquired, had it not been for that discipline which results from studying the practical observations of others. This sentiment from gray hairs may probably have some weight with those who think that every thing is to be gained by the *sight* of diseases, and little or nothing from *reflection* excited by reading. Not a day passes—not a day has passed for twenty years, that we have not seen the most outrageous errors committed by men who pride themselves on never consulting any thing but their own *experience*. Such men were born in darkness—live in darkness—and will die in darkness.

From the Lancet.

ON THE FUNCTIONS OF THE DIFFERENT PARTS OF THE ORGAN OF HEARING. By Dr. CH. L. ESSER.*

This paper is the extract of a work to which the prize was adjudicated in 1825, by the Faculty of Medicine of the University of Bonn.

The cartilage of the external ear appears to contribute very little to render the sounds more distinct, but it serves to increase their force; not only by reflecting a part of the vibrations into the meatus auditorius, especially those which fall into the concha, but also by means of the vibrations which the undulations produce, and which are transmitted to the membrane of the tympanum. It is not correct, therefore, to suppose, (as M. Itard does,) that the external ear is of no service to man in hearing.

The bones of the head do not contribute less than the external ear to the propagation of sounds, which does not take place solely through the medium of nerves, as some authors have supposed, (Treviranus, Swan, &c.) for, in such a case, a watch applied on the cheek, ought to communicate sounds as clear through the medium of the facial nerve, as if it were applied on the zygomatic arch; but this is not the case. The occipital bone is more adapted to the propagation of sounds, than the bones at the anterior part of the head; this is explained by its connexions with the labyrinth, and by the vicinity of the mastoid cells. The use of these cells is not to impede the echo in the interior of the ear, as M. Treviranus supposes; this function belonging to the Eustachian tube.

In several animals, the bones which sur-

* Kastner's Archiv. für die ges. Naturlehre; tom. xii. 1er. cah. 1827, p. 52.

round the labyrinth, and those of the head in general, present arrangements very favourable for the propagation of sounds, in this way compensating for the absence of the external ear.

The meatus auditorius externus is evidently the part which contributes most to concentrate and transmit the sounds to the membrana tympani.

The membrana tympani is put into vibration by the undulations which reach it; a point doubted by M. Itard, although all other authors are agreed on it; this is, however, not the only use of this membrane; for the vibrations of sound can arrive at the internal ear without the assistance of it, and even with greater force; another function of the membrana tympani, is to protect the internal ear from external agents. The experiments of the author, and the facts which he relates, leave no doubt on this subject. The hypothesis of Antenrieth and Kerner, according to which the membrane of the tympanum is considered as an assemblage of cords differently stretched, in proportion as the membrane is round or oblong, is shown to be without foundation. The distinction of the different sounds does not rest on a mechanical arrangement of the ear; but on a psychological cause.

The Eustachian tube is the principal auxiliary of the membrane of the tympanum, and performs four different functions; first, it allows the air contained in the cavity of the tympanum to be placed in equilibrio with the external air. If this equilibrium be disturbed, derangements in the functions of hearing take place, such as the tinkling and stopping up of the ear. If the quantity of air contained in the cavity be increased by deep expirations, great pressure is made on the membrana tympani, and on the other parts of the cavity, especially the fenestra rotunda; this pressure is caused by the stopping up of the ear, which diminishes in proportion as the equilibrium of the air is re-established by means of the Eustachian tube. If the air in the cavity of the tympanum be rarefied, and the Eustachian tube shut from the effect of spasm, the external air presses on the membrana tympani, penetrates through its pores, and the tinkling is produced through this passage. The two phenomena disappear as soon as the equilibrium of the air is re-established in the cavity of the tympanum, which is effected by pushing the air towards the Eustachian tube, the mouth and nose being firmly closed; or by introducing the little finger very deeply into the meatus auditorius externus, and by gradually drawing it back, and pressing from below to above against the wall of this canal. Thus a vacuum is formed, the membrane of the tympanum is inclined towards the meatus auditorius externus, and the Eustachian tube gives passage to air from the back of the mouth. It is evident that this explanation cannot apply to all the phenomena which occur in the organ of hearing. Cerebral congestions, or derangements of nervous action, are the causes of more chronic sensations.

2d. The second function of the Eustachian tube is to allow the air contained in the cavity to be put in vibration; which could not take place if it were closed. In deafness from the obliteration of the Eustachian tube, the perforation of the tympanum is a means of cure, by re-establishing the communication of the tympanum with the external air. The opinion that the walls of the Eustachian tube are constantly in contact with each other is incorrect.

3d. The Eustachian tube prevents any confusion in the vibrations of air contained in the tympanum; and lastly, it serves to conduct the mucus secreted in the tympanum, and by its parietes, into the posterior nares.

The bones of the ear, by the assistance of their muscles, draw the membrana tympani in different directions; but it would be difficult to explain how, and for what reason, this effect takes place; their influence on hearing is not very clear; they serve to transmit the vibrations of the membrana tympani to the fenestra ovalis, although this is not their only use. Some physiologists, Treviranus, and others, deny the conducting property of these bones, because they found in hares a red and gelatinous mass, surrounding the ossicula; and which, according to them, is peculiarly fitted for the transmission of sounds; but this red mass is nothing else than effused blood, which is found only in those hares killed in the chase, whilst no trace of it is observed in those which are decapitated. The sac, in which this mass was contained, according to these authors, was, perhaps, the tendon of the stapedius. The bones of the ear, in some animals, are hollow, and thus appear more fit for the transmission of sound.

The labyrinth, although well understood in its anatomical relations, is, and will, probably, always remain the most obscure part of the organ of hearing, as to its real use. The experiments which the author has made on the existence of the fluid of Cotugnius, are not decisive, and it remains doubtful if this liquid exists during life, or whether it is merely produced after death.

The vestibule or membranous sacs which exist in some animals, and the *semicircular canals*, appear to contribute most to the sense of hearing; but it is difficult to say in what manner. By looking at comparative anatomy, we find that the principal utility of semicircular canals is to strengthen the sounds. They are very much developed in those animals which have no external ear, or which is badly formed, (for instance, birds, mole, man,) those semicircular canals which are well developed, are generally accompanied with a small cochlea, and *vice versâ*. Man is the only exception from the equilibrium between these two parts. The cochlea appears to be of less importance than the semicircular canals; for it soon disappears in the animal kingdom, and birds only possess a rudiment of it; it presents more varieties than the semicircular canals; its principal use appears to be that of presenting a greater surface for the vibrations of sounds, and of con-

centrating them, and in this way giving them power. The opinion of several authors, that the cochlea serves to distinguish the quantity and quality of sounds, does not agree with the results which comparative anatomy furnishes.

The distinction between different sounds is a function purely psychological. If the development of the cochlea indicates the development of the faculty of distinguishing the different species of sounds, the following is the

order in which it is found in animals and man: 1st. The porcupine has $3\frac{1}{2}$ spiral turns? 2d. the dog and fox, 3 turns: 3d. man, cow, hog, and cat, $2\frac{1}{2}$ turns: 4th. the horse and dolphin, $2\frac{1}{4}$; and the rabbit, 2 turns: birds occupy the last place in this series.

The part which the *auditory nerve* takes in hearing is undoubtedly of the greatest importance; but the philosophy of its functions will always remain a mystery.

Medical and Philosophical Intelligence.

On the Circulation of the Blood, and the causes of Absorption. By FERD. LAU, London.—There is no object in physiology that deserves more attention than the phenomenon of the motion of the blood in the animal machine. Physiologists have made the most zealous inquiries on this important subject, and many useful results have been obtained. But it must be confessed, that we do not yet possess a satisfactory explanation, as to all the powers that make the blood circulate. We know that the left ventricle of the heart pushes the blood through the arterial and capillary system; but we cannot conceive, that the same power is alone sufficient to propel it up again to the right side of the heart; still less do we understand how the blood of the vena portæ is a second time brought into a capillary system. Our researches must, therefore, be directed to detect some other additional power, besides that of the heart. One of the most important discoveries which has been made, relative to the circulation of the blood, is that of absorption by the veins, which is now established beyond all doubt. It is a general opinion that this absorption is effected by the diastole of the right heart; but this is contradictory to the laws of muscular action, which we only know to be contractive. But there is another process going on, which, in my opinion, must have an influence on the absorption of the blood in the veins, and which, as far as I know, has hitherto altogether escaped the notice of physiologists. Le Gallois, Rudolphi, and others, have observed, both in man and animals, that the glottis alternately shuts and opens; but Professor Mende, of Göttingen, had a favourable opportunity of accurately observing this action. He was called to the assistance of a man who, with the intention of suicide, had divided the larynx in such a manner that the glottis lay quite bare to view. He was so struck with the motion he observed, that he showed the curious fact to one of his colleagues, who was equally surprised to see what he never could have expected from the appearance of the parts in a dead body.

Professor Mende published a little treatise* on this subject, in which he says that the

glottis is closed, and opened, alternately, by two bodies resembling the lips, in the act of shutting of which there seems to be some degree of power; and to make the closure still more complete, the epiglottis lays down on the glottis; likewise, as if by some muscular action, the closure takes place between each inspiration, and expiration, and lasts longest after the expiration.

It appears, then, that during the closure of the glottis, the communication between the air-canals of the lungs, and the external atmosphere, is entirely suspended; or that the closure is air-tight, which is also particularly favoured by the labial structure of the parts. Now it is my object to show, what the effect of this closure of the glottis must be on the circulation of the blood. We know that a part of the air is absorbed by the blood in the lungs; and are we, therefore, not entitled to conclude, that a vacuum is formed in the bronchi during the closure, and must not such a vacuum evidently have a powerful effect on the fluid blood, which thus, by atmospheric pressure, is compelled towards the vacuum? The pressure of the atmosphere chiefly acts upon the abdomen; the moveable contents of which, and the blood of the large veins, are pressed into the pectoral cavity, until the glottis is opened again, when the equilibrium is restored. This reasoning, I think, is founded on such indisputable principles, that there will be no difficulty in conceiving it. However, I consider the subject of such importance, that I strongly recommend it to the investigation of able men. It is now more than two years since I took this view of the circulation of the blood; and it appears to me, that it throws a light on many obscure points in physiology, pathology, and the action of medicines. —*Lancet*.

*On the Effects of Cupping Glasses on the Development of Vaccine Pustules.**—The members of the Académie Royale de Médecine have been, for some time, endeavouring to prove the effects of exhausted glasses on virus and poison inserted into the skin, but they do not appear to have yet arrived at any satisfactory conclusion. Did Dr. Barry's theory of

* Mende, von der Bewegung der Stimmritze.

* Journ. Gén. de Méd. Mars, 1828.

absorption lead to so useful a point in practice, as to enable us to arrest the progress of poison inserted into the skin, it would be worthy of some attention, however contrary to the laws of physic it might appear. Dr. Barry, conceiving that the absorbent fluids are driven up towards the heart by the weight of the atmosphere on the surface of the body, was naturally led to suppose that, by depriving any part of that surface of atmospheric pressure, no absorption could go on in that part, whilst so deprived. Facts, so far as they have yet been collected, tend to support this opinion, although they prove no more than that the fluid in the absorbent vessels is propelled by the weight of the superincumbent atmosphere, than the flow of blood out of a punctured vein proves that the atmosphere *attracts* the fluid, against its own gravity, out of the punctured vessel. The committee appointed by the Académie to examine Dr. Barry's experiments, found that no absorption went on under a vacuum; that, in fact, if poison be inserted into the skin, and a cupping glass, with the air exhausted from it, be applied over the part, the absorption of that poison into the system is prevented. M. Itard, wishing to satisfy himself of this fact, repeated the experiment, not by inserting poison, as Dr. Barry did, but by inoculating the part with the vaccine virus. He vaccinated a child on the shoulders by several punctures, over some of which he applied glasses, whilst the rest were left uncovered. The punctures which were covered by the glasses formed no pustules, whereas all those which had not been so covered, gave rise to vaccine pustules, possessing the usual characters. M. Bousquet was not satisfied with the result of M. Itard's experiment, so that he determined to repeat it on a large scale, at the bureau of public vaccination of the Académie. M. Bousquet obtained a very different result from that which M. Itard had obtained. In the experiments of the former the glasses appeared to have scarcely any sensible effect in preventing the puncture from forming a pustule. It appears to be the general opinion among the members of the Académie, that a vacuum produced over the puncture may prevent the action of poison on the system, which generally takes place very suddenly, and in a degree proportioned to the dose; but that it is different with regard to a *virus*, which has the property of reproduction, and whose action is slow.

This subject appears to us easily explained. The application of an exhausted glass to a part will suspend the function of absorption in that part while it remains on. It acts in two ways in doing this; first, by the pressure of its edge on the absorbing vessels running from the part, thereby obstructing the motion of their contents; second, by *expanding these vessels beyond their natural caliber*, thereby suspending their function for the time. The absorbents are naturally intended to sustain the weight of the atmosphere, and when this weight is removed, they necessarily expand, and assume a caliber which is not natural to

them. The arteries and veins do the same. It cannot be expected that the absorbents, while in this state, can bring their sides together in order to press forward their contents. Let it be tried whether a portion of the intestinal canal will be able to contract so in a vacuum. As the vacuum of a cupping glass suspends the tonicity or contractility of the blood-vessels where it is applied, nothing can be more probable than that it suspends that of the absorbent vessels, whose coats are much more delicate than those of the arteries and veins. The fact appears to be, that the pressure of the atmosphere is natural to all the vessels, as it is to all other terrestrial objects, and that it is one of the causes which determine their physical form; it is also one of the causes which enables them to perform their functions, but it does not follow from this that they act like syphons, more than it does that the intestinal tube acts so.

Now, it is not only possible, but also probable, that all the poison, or even virus, may be extracted in some instances, if an exhausted glass is applied *immediately* after its introduction—that is, if the glass is applied before the poison or virus has entered the extremities of the absorbents. It may, by this means, be washed out by the blood. But should it once enter the extremities of the absorbents, nothing but a retrograde action of these vessels could discharge it again; for, as the edge of the glass extends all round to some distance beyond the wound, and as there is no pressure between the glass and the wound to force the poison back, it must necessarily remain at rest there, unless it can run back under the law of gravity. The only chance of abstracting the poison when it has once entered the extremities of the absorbents would be, by using a very small glass, which would barely extend beyond the edges of the wound. It is generally supposed, that, because an exhausted cupping glass extracts blood from a puncture, it must also extract the contents of the absorbents, or any thing else which may be in the wound. But this is by no means the case. The blood is constantly forced by the action of the heart towards that part, as well as towards others; it therefore flows out at the puncture. But as there is nothing to force the contents of the absorbents in a retrograde direction, that portion of the fluid situated between the edge of the glass and the wound will remain at rest, but the fluid situated between the glass and the heart may, and probably does, flow on in its natural course. It is true that the absorbents running into the wound from the distal side of the glass may pour out their contents, but, it is not probable that any of the poison enters the divided extremities of these.

Now, it would be advantageous to apply immediately an exhausted glass over the bite of a rabid animal, in order to suspend the absorption of the virus until instruments could be procured, and the consent of the patient and his friends obtained, to have the part excised. The absorption of any other virus or poison might be suspended in the same way for a pe-

riod, to allow time to decide upon the best plan of treatment to be pursued. But should excision be considered necessary, all the parts which have been covered by the glass should be removed, before the patient can be considered safe.—*Lond. Med. and Surg. Journ.*

On the Nature of Continued Fever, and the changes which the Blood undergoes in the progress of that Disease.—Dr. Reid Clanny, of Sunderland, has published a lecture which he delivered not long ago at the Sunderland Infirmary, on the composition of the blood in typhus fever, and on the light which his analytical researches throw on the nature or proximate cause of fever generally. The statements of facts contained therein is much too novel, and, if correct, important likewise, not to deserve all possible publicity. But they are brought forward by the author in a manner far too bare and authoritative, and are in themselves a great deal too extraordinary to allow of us placing much confidence in the results till they are supported by the details of his own experiments, and confirmed by the experience of others.

According to Dr. Clanny the watery part of the blood increases in proportion during the progress of continued fever, while the proportion of each of the solid parts diminishes; and when the crisis has taken place the opposite change commences, so that ere long the blood returns to its former condition. Dividing the period of an ordinary case of mild typhus, or (as we should advise him rather to call it) synochus fever, into three stages of six days each, the first being the stage of increase, the second that of formation, the third that of declension,—he says he has found from the average of many experiments the following to be the proportions of the chief principles in a thousand parts of blood at the close of each stage.

	In Health.	1st Stage.	2d Stage.	3d Stage.
Water, -	678	729	772	732
Colouring Princ.	160	136	122	130
Albumen, -	121	98	73	101
Fibrin, -	28	52	22	26
Salts, -	13	12	9	11

From this table it appears that all the animal principles, as well as the salts of the blood, decrease materially in quantity as the fever advances, and increase again as it recedes; and the author farther alleges that the same changes do not occur in other febrile disorders.

Another change which he says he has detected is a diminution in the quantity of carbonic acid contained in the blood. In health, blood contains, according to his experiments, a sixteenth of its volume of that gas. In the advanced stage of unfavourable cases of typhus it does not contain any; and in the intermediate periods the proportion is found to decrease gradually, but he does not mention in what ratio.

Dr. Clanny infers from these premises that contagious fever is in essence nothing else

than a stoppage of the process of sanguification; and he supports this view by some ingenious arguments drawn from the features of the fever itself, and from the phenomena of analogous diseases. We are not quite prepared to pronounce his premises false; but our own observations certainly lead us to suspect as much. With regard to the diminution of the carbonic acid in the blood during the progress of fever, we must recal the author's attention to the late explicit denial by Dr. John Davy of the statements of Mr. Brande, Dr. Scudamore, and others, as to the presence of *any* free carbonic acid in ordinary circumstances. Dr. Davy could not find it. We have repeated his experiments with the same result, and believe we have also discovered where Brande, Scudamore, and Dr. Clanny found *their* carbonic acid. But even although Dr. Clanny's analysis of the blood in fever should prove correct, that is merely a condition necessary to the existence,—and not evidence of the validity,—of his theory of the nature of fever; for in imputing the febrile symptoms to the absence of sanguification, he commits the very error with which he has charged such theorists as Clutterbuck and Broussais; namely, he mistakes the effect for the cause. If inflammation prevails during life in the head or abdominal viscera, and leaves traces of its ravages in the dead body, that is no proof of local inflammation being the cause of the general fever. In like manner, if a starving of the blood occurs during fever, all the functions of assimilation being suspended, that fact certainly is no proof of the starved blood being the cause and not the effect of the disease.—*Ed. Med. & Surg. Jour.*

Puncture of the Pericardium.—Dessault performed this operation about twenty years ago; the patient died, and upon dissection, it was ascertained that he had mistaken the nature of the disease. Since his time, Laennec and other authors have recommended the operation. At the present day, the precision of our means of diagnosis, enables us to recognise the cases in which an operation might be indicated, but I am not aware that it has been attempted by any surgeon since Dessault. The following case in which it was performed, is detailed in the *London and Paris Observer* for March 11th, 1827: I have not found it recorded in any of the English medical journals. The subject of the case, — Skinner, a girl æt. 14, residing in White street, Cartergate, had been attacked with rheumatism the preceding January. The surgeon in attendance, Mr. Jowett, of the parish of St. Mary, ascertained by means of the stethoscope, that the pericardium and inner membrane of the heart were inflamed. The remedies employed on this occasion relieved the disease, and the patient appeared to be convalescent. The stethoscope, however, together with other indications, evinced the existence of dropsy of the pericardium. On the 13th January, the state of the patient

changed greatly for the worse, and the day following it appeared impossible that she could survive the night, unless something were done for her relief. The operation having been proposed and assented to, was performed that afternoon in presence of Dr. Manson, who had been called in consultation. It was at first proposed to extract the fluid by means of a suitable apparatus, but an accidental circumstance occasioned it to be extravasated into the cavity of the pleura, where it was soon absorbed. In the first twelve hours after the operation, there was a sensible amendment, and although the patient was in a state of great debility and exhaustion, strong hopes were entertained of her ultimate recovery.—*M. de Fermon. Bull. des Sciences Médicales.*

Employment of Stramonium in different Nervous Affections.—Professor Wendt supposes that the action of stramonium, though in other respects sufficiently analogous to that of belladonna, is distinguished from it, in operating less upon the circulatory system, and in producing singular effects upon the organs of generation. Taken in a large dose, by an individual in good health, it excites the venereal appetite; while, given to a person labouring under this species of excitation, it soothes it by a homeopathic action. Cases of sur-excitation therefore, of the genital organs, are those in which it is particularly indicated; it is also useful, according to Professor Wendt, in all the phlegmasiæ accompanied with many nervous symptoms, in affections of the internal genital system of the female, in nymphomania, in epilepsy, occasioned by onanism or any irritation whatever of the generative organs, in myelitis, œsophagitis, and carditis. In all cases, the inflammatory diathesis, if present, should be removed, before resorting to the administration of the plant in question. Professor Wendt prefers it in the form of tincture, prepared as follows:—Two parts of the bruised seeds; malaga wine, eight parts; alkohol, one part; digest at a gentle heat, express, and filtre. The dose for an adult, is from five to twelve drops, every two hours.—*Rust's Magazine.*

Instance of Obliteration of the Aorta opposite the Fourth Dorsal Vertebra. By PROFESSOR MECKEL.—A peasant, aged 35 years, previously in good health, robust, and well-made, was, all at once, on the 18th January, seized with a sense of great debility, while carrying a sack of grain to market. He was carried to the hospital immediately. The symptoms of syncope and vertigo were dissipated in a few hours; to which succeeded gastric irritability, pain in the chest, total loss of appetite, bilious vomiting, the pulse remaining little altered. By the sixth day, the patient appeared to be completely cured—got up—and was walking about—but suddenly fell down dead.

Dissection.—On opening the thorax, the pericardium was observed to be filled with black blood, occasioned by rupture of the

right auricle, which was softened in its structure. The aorta ascendens was found to be too much dilated for injection from that point—and, therefore, ligatures were thrown on the left subclavian and carotid arteries, while the tube was fixed in the arteria innominata. The injection was considered to be unsuccessful, and as the subject had been designed for a demonstration, it was thrown aside. On opening the abdomen, afterwards, the vessels were seen injected, as were those of the lower extremities down to the feet. The examination being prosecuted, they found the aorta, immediately below the arterial ligament, reduced to the size of a crow-quill, while a beautiful net-work of vessels was seen between the trunks, going off from the arch of the aorta, and the intercostals of the aorta descendens. The said intercostals were very much enlarged, and had produced grooves in the ribs. From this circumstance it was inferred, that the obliteration of the aorta was an affection of long standing, and could not possibly have dated from the late attack of syncope, six days previously. The man must, therefore, have not only survived the cause of the obliteration, whatever it was, but lived in good health for many years afterwards. On inquiry, all that could be learnt, was, that this man had been very often ill in his youth; but afterwards had grown up strong and muscular.—*Journal Complémentaire.*

M. Dupuytren's Treatment of Phagedenic and Corroding Herpes.—There is not a physician who has not had an opportunity of observing and treating phagedenic or corroding herpes, and to experience a disagreeable proof of the inefficacy of the anti-herpatic, anti-scorfulous, anti-venereal remedies, and others which have been tried by turns against this cruel disease, according to its different appearances, and its supposed nature. We know, that in spite of all the remedies, the phagedenic herpes eats and destroys no less the nose, the lips, the cheeks, the eyelids, the ears, the temples; parts which it more especially and frequently attacks. Fire itself seems to irritate, as well as arsenical paste; these agents have besides the inconvenience of destroying the parts on which they are applied, and to add to their deformity. These motives have, for a long time, induced M. Dupuytren to seek other remedies against phagedenic herpes, and it seems certain, that they may be cured without deformity, by the use of the following powder:—

℞ Hydrarg. Submur. præcip. partes 199
Oxidi. Arsenici. Albi. vel } partem - 1
Acidi. Arseniosi.

200

This remedy, which acts rather as a specific than as a caustic, may be variously employed. If the surface of the herpes is ulcerated, moist and cleaned, it is powdered with a little puff, charged with the above described powder, so as to cover it with a thick layer, of about the

twentieth part of an inch. If this surface is covered with a scab, it must be thrown off by means of a poultice, and then it is powdered as has been just described. In fine, if the herpes is actually covered with an imperfect cicatrice, it must be destroyed; twenty-four hours after, the surface is powdered, when it must necessarily have ceased bleeding.—*Medical Guide to Paris.*

M. Jadelot's Treatment of Croup.—M. Jadelot considers the croup as a kind of angina of the air passage, presenting more violent symptoms, and having true paroxysms separated by well marked intermittents of special character. He admits different degrees in the disease according to its intensity, but without changing opinion as to its nature. Bleeding by leeches, and emetics, are the agents the most employed in the treatment of croup. The emetic alone has often sufficed to stop the disease, especially when it takes place in weak, pale, and bloated subjects; but in the opposite cases he insists on the application of leeches, and allows the blood to flow long enough for the infant to become pale, and the pulse to lose its strength. If the bleeding be too soon stopped, there is a danger of not arresting the progress of the evil, and a result, which is at least troublesome, is, that of being obliged to apply more leeches.

After the bleeding, M. Jadelot causes vomiting, several times in succession, at intervals of two or three hours, and the practice is attended by the greatest success, for the children find themselves relieved each time that they have vomited.

When the croup has arrived at the second period without having been opposed, and the presence of a false membrane is suspected, M. Jadelot directs leeches to be applied, but from the moment that they fall off he hastens to produce vomiting, and it is in this case that he employs by spoonfuls, every ten minutes or quarter of an hour, the mixture called *anti-croupal**, until he has obtained vomiting. He insists equally upon derivatives used upon the skin or in the intestinal canal; he advises also to provoke sneezing.

When the disease is very rapid, it has been a question whether we ought to commence by bleeding or emetic. M. Jadelot's opinion is, that we should bleed first if the infant be robust, and if it present signs of congestion towards the superior parts; on the contrary, he would commence by vomiting, when the subject is pale and exhausted, and there is little heat and fever.

Symptomatic Diabetes Mellitus.—It is an important fact in the pathology of this mysterious disease, that when the system is gradually sinking under phthisis, one of its common-

* Anti-Croupal mixture—℞ Infus. Polygalæ ℥iv.; syr. ipecacuanah ℥j.; oxymel. scillæ. ℥iij.; antim. tart. gr. jss. misce.

est terminations, or when life is cut short more abruptly by some other supervening disorder, the morbid secretion sometimes returns to its healthy state,—a proof that the elaboration of saccharine matter in the kidney is not owing to an organic change in structure, but simply to derangement of function. A new and singular fact of the same nature has been detailed by Dr. Bennewitz in Osann's Clinical Report for 1823-4-5; namely, the occurrence of diabetes mellitus in conjunction with pregnancy. The case is in many respects interesting. A stout young woman who previously had three children, and always carried her child to the full time without any material disturbance of her health, became pregnant for the fourth time. During the whole of that pregnancy she was tormented with insatiable thirst, and profuse discharge of urine; but as she had no other complaint of sufficient moment to attract her attention, she did not apply for advice; and the nature of the urine was never ascertained, as the quantity of liquid she drank was naturally thought to be a satisfactory explanation of the increased quantity of liquid discharged. The thirst and diuresis ceased suddenly soon after she was delivered, and she recovered perfectly. At the age of twenty-two she became pregnant for the fifth time; and hardly had the pregnancy begun when the thirst and diuresis reappeared even in a more tormenting degree than before; no other symptom of ill health, however, accompanied them, so that it was not till the seventh month that she applied for medical advice, and even then thirst was her chief complaint. The desire for drink, caused by a burning and itching sensation in the throat, was such that she drank daily five or six Berlin quarts; but her hunger was not preternatural. Her digestion at the same time was vigorous; and although she said she had been much stronger at the commencement of her pregnancy, she was still a stout looking woman. The urine considerably exceeded in quantity the liquid drank, amounting in fact to eighteen medicinal pounds; it was watery and muddy; had a faint smell like stale beer; and had a taste resembling that of beer, but much sweeter. The tongue was clean and dark-red; the mouth constantly dry; the gums shining, red, and retracted, so that the teeth were loose; the voice weak and hoarse; the bowels regular; her sleep disturbed by calls to drink; the skin rough, harsh, dry, never perspirable; the pulse full, hard, and frequent; the temperature of the body irregular;—and menstruation had continued during the whole period of her pregnancy. Latterly she had also pains in the loins, shooting towards the pubis, and particularly troublesome when she walked. No symptom whatever could be detected of a local affection of the kidneys. On account of the state of the pulse twelve ounces of blood were taken from a vein; but no change whatever was caused in the symptoms. The blood drawn formed an abundant dark-red crassamentum without sizyness, and a clear serum of a peculiar faintly-sweetish smell and taste. A strict animal diet and warm clothing were then

enjoined, together with the occasional use of magnesia and hyoscyamus to keep the bowels moderately open; but although she in consequence seemed to feel more comfortable, the state of the urinary secretion remained unaltered. About this time it was analyzed by *Hermbsstaedt*, and found to contain *two ounces of saccharine matter per pound (civilpound.)* At length the labour pains commenced prematurely, (the precise time not mentioned,) and she was delivered of a female infant weighing twelve pounds, and which died in the passages. Next day she was attacked with great weakness, tearing pain in the lower belly, so acute that she could not bear the pressure of the bed-clothes, delirium, flushing of the countenance; but as the lochia continued to flow naturally, she was not subjected to any particular treatment. Next day, however, the same symptoms continuing, leeches were applied to the abdomen, and a laxative administered. The operation of the latter was followed by profuse perspiration, the first she had had since her pregnancy began. The inflammatory symptoms then rapidly disappeared; at the same time the thirst, diuresis, and saccharine taste of the urine became less and less, and she was soon restored to perfect health. The urine was carefully analyzed again by *Hermbsstaedt*, (at what distance of time after delivery is not stated,) and he could not detect in it any trace of sugar. Six months after being dismissed cured, she became pregnant a sixth time. The relater did not see her during her pregnancy; but he was subsequently informed by her that she had the same thirst, heat in the throat, and diuresis, though in a much less degree than formerly; and that in addition she had a profuse *fluor albus*, which no treatment could check during her pregnancy, but which ceased of its own accord soon after delivery.—*Ossann's 12ter Jahresbericht des Poliklinischen Institutes zu Berlin*, p. 23.)—*Ed. Med. and Sur. Journal.*

Acetate of Morphine.—A letter by Dr. Marroli is inserted in the *Annali Universali di Medicina*, detailing a number of cases in which this preparation was successfully employed; in one instance, a stout and vigorous man had been attacked on three succeeding years, at the same period, by a spasmodic pain about the middle of the humerus, unaccompanied by redness or tumefaction. The first attack yielded to a plaster, the composition of which was unknown, and the second to leeches, cupping, tartar emetic ointment, blister, &c. In the third, half a grain of the acetate mixed with sugar, and divided into two doses, was directed to be taken during the 24 hours. After the second dose, the pain disappeared entirely. A case is also detailed of a woman, who had been tormented for several days with lacerating pains in the left arm, extending to the corresponding mamma, and the sternum. She was unable to move her arm; there was continued fever, thirst, &c. V. S. thrice repeated, purgatives, frictions, with volatile linament, and finally the acetate of morphine

was directed. The pains were greatly mitigated after the first dose, and almost entirely disappeared after the second. Exostosis of the sternum, revealed an inveterate syphilitic affection, the usual remedy was employed, the exostosis subsided, and the patient recovered the use of her arm as before.

We cannot afford room to all the cases of Dr. Marroli, which indeed do not possess a very high degree of interest, except, inasmuch as they tend to diffuse a knowledge of a remedy which has not hitherto attained the eminence it deserves. Several examples are given of its efficacy in spasmodic affections of the uterus, attended with fever, vomiting, &c. &c. We can only subjoin the following detail of a different disease.

A man, æt. 50, of a good constitution, was attacked with severe pain of the left side of the neck and cheek, accompanied with strong pulsations of the carotids, flushed countenance, anxiety, palpitations of the heart, pulse hard, vibrating, &c. V. S. was seven times repeated during six days, leeches repeatedly applied, purgatives, digitalis, oxide of bismuth, diaphoretic beverages, &c. were employed, and the patient appeared convalescent, when there suddenly supervened a rigour which lasted several hours, followed by anxiety, intense thirst, high febrile action, profuse perspirations, and violent pain in the neck. Six grains of the sulphate of quinine, made into eight pills, were directed to be taken, one every two hours; the disease yielded to this remedy, but re-appeared some time since, and besides the symptoms above enumerated, the patient complained of a pricking sensation at the spot where the leeches had been applied the preceding year. One grain of the acetate of morphine was divided into four parts, one to be taken every twelve hours; there was a sensible diminution of pain after the first dose, and it disappeared entirely after the second.

Cancer of the Penis.—At a meeting of the Institut Royal, M. Delpech announced that he had recently removed the penis in a case of cancer involving the whole of this organ. The disease penetrated beneath the symphysis of the pubis, and could not, therefore, be entirely removed by amputation, while on the other hand, it would have been very difficult to pass a ligature around the vessels under the arch. The extirpation was effected by M. Delpech, by making in the first instance an incision, which divided the scrotum into two parts, extending to the root of the penis; the deep seated parts were thus brought into view, and the operation completed. The division of the scrotum was afterwards maintained complete by means of sutures, which united the skin to the deep seated parts, leaving after cicatrization a profound channel, which afforded a ready passage to the urine, the want of which is the cause of so much inconvenience when the penis is amputated near its root.

In a note to the above, M. Gendrin observes, that the separate cicatrization of the

two portions of the divided scrotum has its advantages, and is an improvement due to M. Delpech, but the necessity of this division, in order to extirpate the roots of a cancer of the penis extending beneath the symphysis, was demonstrated by M. Dupuytren some years since, and has been twice performed by that celebrated surgeon.—*Jour. Général de Médecine, &c.*

Metritis cured by the Hydriodate of Potash.

—A woman, æt. 42, complained, two days after a difficult accouchement, of severe pain in the uterus, extraordinary sensibility of the abdomen, and frequent desire of urination. The uterus was still considerably distended, the vagina hot and dry, the lochial discharge very small in quantity, fever not very high; the secretion of milk had already begun. The bowels were evacuated by an injection, and M. Guerard prescribed three grains of the hydriodate of potash in six ounces of emulsion, to be taken during the 24 hours. The following morning, the uterus had contracted, the pains disappeared, the lochiæ were re-established, and the patient appeared so well that the medicine was discontinued. The iodine had no influence upon the lacteal secretion, nor upon the health of the child.

Case 2d.—Madame F. after much suffering, was delivered by the forceps in her first accouchement. The second day, inflammation of the uterus supervened, it had only slightly contracted, and was very tender to the touch; there was no lochial discharge; the other functions were scarcely disturbed. Dr. G. prescribed the hydriodate of potash as in the preceding case, and with similar success, for after the lapse of twenty-four hours the patient was convalescent.—*Horn's Archives.*

Paralysis from Cubebs.—Mr. Broughton has related a curious case of this kind, in the person of a young gentleman, who had been taking the above mentioned medicine for about a fortnight, in the dose of two drachms thrice daily, for a gonorrhœa, and being otherwise in good health, and living quietly. A distortion of the mouth to one side, whenever he attempted to speak or smile, supervened—and the pulse became irregular, with some other symptoms of constitutional derangement. He was bled to half a pint, without any advantage. Then he was freely purged, with benefit, but at the end of three weeks the distortion had not entirely disappeared. Was this not an affection of the portio dura, and the result of cold caught in the side of the head, rather than the effect of cubebs?—*Med. Gaz.*

Employment of Pyrothonide in Angina*

* "This is the name given by Dr. Ranque, chief physician to the Hôtel Dieu, at Orleans, to a product obtained by the combustion of hempen, linen, or cotton cloth. As it is coming into notice for chilblains, chronic inflam-

and other Inflammations.—Dr. Ranque has made a happy application of this substance to the treatment of Angina, whether simple, or complicated with scarlatina or measles. In simple angina, the resolution of the inflammation has been rapid; it having been sufficient to employ, ten or twelve times a day, a gargle consisting of two grains of pyrothonide to the ounce of barley water, and a little honey. In violent angina, accompanied with scarlatina, he did not venture in the first instance, to confine himself to this gargle alone, but employed concurrently, leeches to the neck and epigastrium, and then the disease was limited to the cutaneous eruption. But subsequently, emboldened by success, he employed only the gargle and a rigid diet, and in a few days, traces of the angina were scarcely perceptible. Reflecting upon its salutary effects in these different cases, he was induced to make trial of it in epidemic angina maligna, and pretends that he has found it equally successful in the treatment of this terrible disease. According to him, pyrothonide in union with barley water and honey, is one of the best resolvents of inflammation, and one of the best solvents of the effused coagulable lymph. He relates three cases of ophthalmia treated successfully by this means, and cites some instances of its employment in syphilitic gonorrhœa, simple or complicated, and in leucorrhœa. The mode of administration consists in dissolving 24 grains of concrete pyrothonide (it is obtained in this form by evaporating the aqueous solution at a gentle heat; it has a resinous aroma, is not acid, is agreeable to the palate, and leaves a sensation of cold on the surface to which it has been applied, after producing a slight excitation,) in six ounces of water, and using it as an injection, three or four times daily, while at the same time, it is employed in form of fomentations. We do not believe that epidemic angina maligna has been so easily cured, as is pretended by Dr. Ranque, although we are willing to attribute some remedial agency to the pyrothonide. What is this substance? evidently simple pyroligneous

mations of the eyes, we give the directions for its preparation. Take a handful of hempen, linen, or cotton cloth, either old or new. Place it in a shallow basin, and set fire to it in the open air. As the combustion proceeds, prevent the basin from heating too much. When it has ceased, reject the carbonaceous residue, and there will be found a semi-aqueous, semi-oily product, of a reddish and brownish tint, and possessing a penetrating but not disagreeable odour. Pour upon this substance a glass of cold water, and spread the water by means of a bottle brush, over those parts of the basin where any of the oil may be formed, with a view to its complete solution. In this manner, a liquid is obtained of a tint more or less deep, according to the quantity of the substance, produced by the combustion and dissolved in water."—*North American Med. and Sur. Jour. from the Nouv. Biblioth. Med.*

acid, notwithstanding all its inventor may say to the contrary; and as the utility of this acid has been recognised in many of these circumstances, it is not to be wondered at, that the use of the pyrothonide has been sometimes followed by salutary consequences.—*Archives Générales de Médecine.*

Frontal Neuralgia, cured by the Extract of Belladonna. By Dr. LECLERCQ.—A gardener, æt. 27, possessing a strong constitution and sanguineous temperament, had been habitually obliged to submit to venesection several times a year, for the relief of violent pains in the head, to which he had been subject. Dec. 14, 1827, after having worked for a long time in the cold, he was attacked with acute pain in the right frontal region, and the eye of the same side. The pain, which returned about five o'clock every morning, and did not cease till about nine or ten, commenced with a pricking sensation, followed by shooting pains; the eye reddened, became suffused with tears, and was unable to support the light, the eyelids were contracted, and the patient suffered very severely. The pulse was unaffected, and all the functions were performed regularly. About 8 or 9 o'clock, the pain gradually abated, and soon disappeared entirely; leaving a slight heaviness of head, and a kind of stupor. The plethoric condition of the patient, appeared to demand V. S. which was unattended with any advantage. The periodical character of the disease led to the employment of the sulphate of quinine, which was given at first to the extent of fifteen grains, and soon increased to twenty, without any other effect than slightly to retard the accession of the paroxysm. Recollecting to have read in the *Journal Général de Médecine*, a case of sub-orbital neuralgia, cured by M. Lisfranc, by means of frictions around the orbit, with an aqueous solution of belladonna, Dr. L. determined to have recourse to the same remedy. A cloth moistened in a solution, consisting of one drachm of the extract of belladonna, in an ounce of lettuce water, was repeatedly applied to the seat of pain. The first day, no alleviation was perceptible; the second, a sensation of numbness throughout the whole frontal region, &c. was substituted for the pain; and the third day, the cure was complete. The belladonna was continued two days after the cessation of the pain. During eight or nine days, the patient complained that he could only distinguish objects confusedly with the right eye. More than three months had elapsed when the preceding account was drawn up, and there had yet been no return of the disease. This case is interesting; 1st, because it is an additional proof of the efficacy of belladonna in the treatment of facial neuralgias; 2d, because it tends to prove the special action of belladonna upon the fifth pair of nerves, an opinion entertained by some physicians; 3d, because it confirms the statement of MM. Chomel, Cloquet and Bricheteau, who have asserted that they have seen the employment of belladonna followed by tempora-

ry blindness; in the case related vision was affected. From the cases cited by these physicians, it might perhaps be inferred, that belladonna should not be employed for a long period, nor without great prudence, in diseases of the eyes and neuralgias of the third pair, but these apprehensions vanish, when we recollect the success with which M. Demours has employed, for two or three years in succession, the extract of this plant, in order to dilate the pupil, and thus facilitate vision, in cases of blindness arising from opacity of the centre of the cornea or of the crystalline.—*Archives Générales de Médecine.*

Innocuous Nature of Putrid Exhalations.—A committee have been engaged in France in examining the circumstances relative to the knacker's operations. His business consists in killing old worn-out horses, and turning every part of their body to account. The most singular results which the committee have obtained relate to the innocuous nature of the exhalations arising from the putrefying matter; every body examined agreed that they were offensive and disgusting, but no one that they were unwholesome; on the contrary, they appeared to conduce to health. All the men, women, and children concerned in the works of this kind had unvarying health, and were remarkably well in appearance, and strong in body. The workmen commonly attained an old age, and were generally free from the usual infirmities which accompany it. Sixty, seventy, and even eighty, were common ages. Persons who live close to the places, or go there daily, share these advantages with the workmen. During the time that an epidemic fever was in full force at two neighbouring places, not one of the workmen in the establishment at Montfaucon was affected by it. It did not appear that it was only the men who were habituated to the works that were thus favoured: for when, from press of business, new workmen were taken on, they did not suffer in health from the exhalations.

In confirmation of the above observations similar cases are quoted: above 200 exhumations are made yearly at Paris, about three or four months after death; not a single case of injury to the workmen has been observed. M. Labarraque has observed, that the catgut makers, who live in a continually putrid atmosphere, arising from macerating intestines, enjoy remarkable health. Similar circumstances were remarked at the exhumations of the *Cimetière des Innocens*.

Whatever disease the horse may have died of, or been killed for, the workmen have no fear, adopt no precautions, and run no risk. Sometimes, when strangers are present, they pretend to be careful, but, upon close inquiry, laugh at such notions. They handle diseased as well as healthy parts always with impunity. They frequently cut themselves, but the wounds heal with the greatest facility, and their best remedy is to put a slice of the flesh about the wound.

On making inquiry of those to whom the horse-skins were sent, and who, besides, having to handle them when very putrescent, were more exposed to effects from diseases in the skin, they learnt that these men, also, from experience, had no fear, and never suffered injury. Horse-skins never occasioned injury to those who worked them, but in this they differed from the skins of oxen, cows, and especially sheep, which sometimes did occasion injury, though not so often as usually supposed.—*Recueil Industriel*, v. 55.

Analysis of a specimen of Cutaneous Perspiration. By J. BOSTOCK, M. D. F. R. S.—Dr. Bright sent to Dr. Bostock, for analysis, about four ounces of fluid, being the cutaneous perspiration of a patient of his at Guy's Hospital. By various computations (for an account of which we must refer to the original paper,) the following was obtained as the result:—

Water	981. 7
Animal matter	4. 6
Muriate of soda	12.56
Soda	1.14
Phosphates and sulphates a trace	

1000.00

The animal matter was found partly soluble, and partly insoluble, in alcohol. The alcohol being evaporated, afforded a residuum manifesting a certain resemblance to urea, being apparently intermediate in character between this substance and osmazome. The part which was insoluble in the alcohol, resembled most nearly "the substance which forms the principal ingredient of the serosity of the blood." There was a very minute and scarcely appreciable portion of albumen, but no jelly.

It appears that the patient, from whom the perspired fluid was obtained so largely, was a robust sailor, aged 64: he had formerly suffered from gravel, and had slept in damp sheets six days before his admission. Shivering, eructation, vomiting, pain in the belly, and constipation, followed. These symptoms were relieved, after a short time; when he complained of occasional griping pains, and his stools became deficient in bile; his urine pale, and much increased in quantity. After two days more he had pain round the umbilicus and over the pubes, particularly on pressure, or voiding his urine, the quantity of which now amounted to ten pints in twelve hours. Some dysenteric symptoms next showed themselves, for which he took ipecacuanha and hydrar. c. creta. His mouth soon became affected, and the state of his bowels improved; but the quantity of urine continued very large. He was ordered to go into the warm bath twice in the week, and this was followed by perspiration so copious, that it was observed "running completely through the bedding, and forming streams upon the floor." He gained strength notwithstanding, and the urine diminished in quantity, and the patient appears to have got well.—*Lon. Med. Ch. Transactions*.

Analysis of the Cephalo-spinal Fluid. By

M. Lassaigne.—In one of the numbers of this journal, published last year, we gave the results obtained from an analysis of the fluid, contained in the spinal canal of the horse. It was shown that this fluid, which was rather denser than water, contained, independently of the alkaline salts which are found in ordinary serum, very minute quantities of albumen and osmazome.

Desirous of ascertaining the composition of this fluid in the human species, M. Magendie sent me, for analysis, a quantity which he had extracted from the body of an old woman, who had been ill several years, and ultimately died of insanity.

Its specific gravity at the temperature of $+10^{\circ}6$ was 1.0082, and 100 parts furnished water 98.564, osmazome, 0.474, albumen, 0.088, soda, animal matter and phosphate of soda, 0.036, chlorurets of sodium and potassium, 0.801, phosphate of lime, 0.017.

At the request of M. Magendie I compared this fluid with that found in the cerebral ventricles of a man, who, for the last two years, had been affected with general paralysis, and died insane at the Maison Royale de Charenton. Its specific gravity at $+8^{\circ}5$, was 1.0086, and contained in 100 parts, water, 98.738, osmazome, 0.444, chloruret of sodium and potassium, 0.713, soda, animal matter, and phosphate of soda, 0.058, albumen, 0.047.—*Journal de Chimie Médicale, &c.*

Chemical Examination of the contents of a sebaceous tumour, forming an atheromatous cyst. By Professor Nees d'Esenbeck, jr.—The tumour was situated on the great pectoral muscle of a male subject, and equalled in size a large nut; its parietes were composed of three lamina, the most internal, had a horny or epidemic character; the middle, about half a line in thickness, resembled the mucous membranes; condensed cellular tissue formed the external envelope.

The contents of the cyst weighed, when dried, 95 grains, and were composed of pure stearine, 23 grs.; osmozome, 12 grs.; animal mucus, 11 grs.; dry albumen, 23 grs.; phosphate of lime, 19 grs.; carbonate of lime, 2 grs.; carbonate of magnesia, $1\frac{1}{2}$ grs.; traces of elain and of acetate of soda, and loss $3\frac{1}{2}$ grs.

The contents of the cyst may, therefore, be regarded as a kind of emulsion formed by the union of the fat and albumen with the earthy salts.—*Bull. des Sciences Médicales*.

Termination of the Umbilical Vein in the right auricle, and a single umbilical artery arising from the abdominal aorta, in a newborn infant. By Professor Mende.—The child, in whom these remarkable deviations from the natural state were observed, died shortly after birth, without any apparent cause.—There was nothing unusual on the exterior of the body; but, on injecting the vessels, it was found that the umbilical vein did not divide into two branches to traverse the liver, but was continued as a single trunk along the convex surface of the right lobe of this organ,

without being attached to it, as far as the right auricle of the heart, when it opened itself a little above, and in front of the termination of the inferior vena cava. The base of the heart was inclined a little more than natural to the right side, and towards the sternum. There was only one umbilical artery, which arose from the abdominal aorta at its bifurcation; it passed on the left side of the bladder, and continued its course to the umbilicus.

Professor Mende draws from this case some physiological conclusions, relative to the uses which the liver serves in the fœtus. He considers that this organ does not receive all the blood which comes from the placenta; the comparison which has been instituted between the human fœtus and amphibious animals, especially the seal, being incorrect; and that the blood coming from the placenta is not modified by the liver, and does not serve for the secretion of bile, the gall-bladder being, in the case described above, full, and the fœtus well-nourished.

On the Vagitus Uterinus. [*Revue Médicale* Février 1828.]—Although many German writers on obstetrics and medical jurisprudence believe firmly that the fœtus may be sometimes heard to cry in the uterus, and the fact is so generally admitted in the German schools, that several medical gentlemen who have visited this country have stated to us that they have themselves heard it, and all laugh at the idea of its being doubted,—the accuracy of the fact nevertheless continues to be questioned both in Britain and in France, more particularly, however, by the French accoucheurs. M. Lesauvage of Caen, well known to toxicologists, as having some years ago settled the question regarding the alleged poisonous properties of pounded glass, has declared his dissent from the general disbelief of his countrymen in the *vagitus uterinus*, and transmits as his reason the following incident to the Parisian Society of Medicine. A bitch fell sick when far advanced in pregnancy. On approaching her, there was heard distinctly, and even at the distance of ten paces, the cries of her pups, whose movements could be also seen through the abdominal parietes. She did not bring forth her young till two days after, “so that the *vagitus* in this case necessarily supposes the spontaneous development of a gas in the amniotic fluid of each fœtus.”—*Edin. Med. and Sur. Jour.*

Method of discovering Potassa by the Blow-pipe Flame.—M. Harkort, of Freyberg, says, that, in consequence of an observation made by Kirwan, namely, that oxide of nickel with potash, gave a blue glass before the blow-pipe, whilst soda with the same oxide produced a brown glass, he was led to examine whether the distinction might not be made to afford a useful test. On making the experiment with potash, he obtained an excellent result; the blue produced is not likely to be confounded with that produced by cobalt, because it inclines to a milky appearance. So sensible is

this test, that the presence of potash was readily discovered in the periclinite, (a new variety of felspar, distinguished by professor Breithaupt,) although existing there in a very small quantity. The experiment relative to soda was not so successful, the glass acquiring only a weak brown colour.—*Jahr. der Chem.* 1827.

Effect of Electricity on pointed leaves, &c. and on Vegetation.—For the double purpose of ascertaining the power of spines and sharp-pointed lanceolated leaves in modifying the electric relation of the atmosphere and the earth, and in affecting the progress of vegetation by their electric influence, M. Astier insulated a sextuple spine of the *Gleditzia triacanthos* at the top of his house, and brought a wire down from it to an insulated flower-pot, in which were planted five grains of maize; a similar sowing was made in an uninsulated pot, for the purpose of comparison. The experiment continued from the 6th to the 20th of June, including two stormy days. The electrometer gave considerable signs of electricity in the flower-pot, and by using the condenser sparks were produced. The electrified grains were found to pass more rapidly through the first periods of vegetation. When Bengal rose-trees were submitted to the same experiment, the flowers of the electrified plant appeared more rapidly and more abundant than in the other case.—*Ann. Linn. de Paris.*

Preservation of Leeches.—A new vessel of deal large enough to contain sufficient water for five hundred leeches, is to be furnished with a stop-cock to draw off the water. It is to be half filled with the mud from the lake or pond whence the leeches have been taken, and two or three roots of the Florence Iris, (*Calamus Aromaticus*) are to be set in the mud. The leeches like this plant. The usual precautions as to temperature, frequent change of water, &c. are to be taken; the water is to be changed slowly, and the fresh water added by means of a funnel descending to the bottom of the vessel. This method has been found preferable to all others tried at the hospital of Bamberg.—*Bull. Univ. cxiii.* 369.

Change of Crystalline State in a solid Body.—“It was in the sulphate of magnesia that I first remarked the change, in form, of a solid body, or, more accurately, the change in the position of its atoms, without the assumption of the liquid state. If this salt, or the sulphate of zinc, be slowly heated in alcohol, and gradually raised to ebullition, the crystals will lose their transparency by degrees, and, when broken, they will be found to be formed of a great number of new crystals, entirely different in their form to those of the salt employed.”—Mitscherlich, *Annales de Chimie*, xxxvii. 206.

This is a case of internal motion, to be added to those already known of basalt, arsenious acid, barley-sugar, sulphur, &c. &c.—*Quarterly Journal, &c.*

Singular action of Arsenic Acid on Sugar.—When a solution of pure arsenic acid is mixed with sugar, and left for a few hours, a rose colour is produced, which soon becomes a fine purple, and then remains, with little further change, for many days. Sugar of milk, mannite, raisin sugar, sugar of starch, produce similar effects; but sugar of liquorice, diabetic sugar, and such bodies as starch, gum, &c. produce no effect of the kind. Nor do the soluble arseniates or arsenious acid produce these effects with the substances named above.—*Bull. Univ. A. ix.* 281.

The proportion of Morphine contained in Opium.—The actual proportion of morphine contained in opium is not generally known to the profession; and chemists have been very silent upon this subject. But M. Pelletier let out the secret, a few months ago, at the Académie Royale de Médecine. He says, that five hundred grammes* of opium will yield from thirty-six to forty grammes of morphia.

The proportion of Emetine contained in Ipecacuanha.—M. Pelletier informed the Académie, also, of the usual proportion of emetine which ipecacuanha contains. According to him, one livre† of ipecacuanha will yield from twenty-five to thirty grains‡ of emetine.

Vaccination in France.—During the year 1825, there were born in France 587,948 children, of whom 378,500 were vaccinated; 26,571 had the small-pox; of these 2,245 were either disfigured or debilitated by the disease, and 3,369 died of it. The sum of 31,305 francs was expended in the vaccination of about two thirds of the children born in this year.

In the kingdom of Naples, the number of births, for 1824, was 231,936; vaccinations 67,974.

The Russians have recently introduced this salutary practice into California. The chief surgeon, Novitzky, has vaccinated a great number of children, and, also, Governor De Monterey, and his family. In 1823, vaccination was introduced into the Aleutian isles.—*Bull. des Sciences Médicales.*

Method of obtaining the Figure of a Plant.—A piece of paper is to be rubbed over with powdered dragon's blood, in the manner practised by engravers, and then the small branch or leaf, of which the design is required, is to be laid upon it: by means of slight friction, it soon takes up a small quantity of the powder, and being then laid upon moistened paper, an impression is to be taken in the manner practised for lithography without a machine. This process may be usefully employed for preserving certain physiognomical and characteristic

features, which cannot be retained by drying the plant.—*Bull. Univ.*

Compressibility of Water.—Oersted finds, in conformity with the previous experiments of Canton, that water is more compressible at the freezing point than at a higher temperature. At 32 degrees Fahrenheit the compressibility of water is about a tenth greater than 34½ degrees Fahrenheit. At higher temperatures it is still less, but not in so high a proportion.

New Publications.

Deafness; its Causes, Prevention, and Cure. By John Stevenson, Esq. Member of the Royal College of Surgeons, Lecturer on the Structure, Economy, and Diseases of the Eye and Ear; and Surgeon-Oculist and Aurist Extraordinary to his Royal Highness the Duke of Clarence. 8vo. pp. 262. Henry Colburn, London, 1828.

A Rational Exposition of the Physical Signs of the Diseases of the Lungs and Pleura; Illustrating their Pathology, and facilitating their Diagnosis. By Charles J. B. Williams, M.D. 8vo. pp. 191. Underwood, London, 1828.

Dr. Williams has treated his subject in a very able manner. The work is well deserving of perusal.

Traité des Maladies des enfans nouveaux nés et à la Mamelle, fondé sur de nouvelles observations cliniques et d'anatomie pathologique, faite à l'hôpital des enfans trouvés de Paris, dans le service de M. Baron, par C. Billard, ancien interne de cet hôpital, docteur en Médecine de la Faculté de Paris, &c. un fort vol. 8vo.

Atlas d'Anatomie pathologique pour servir à l'histoire des maladies des enfans, par C. Billard, gr. in 4to. de 10 planches, avec le texte explicatif.

Analyse détaillée de l'histoire de la santé, des influences qui la modifient, et des conséquences positives d'hygiène qui en découlent; par P. N. Gerdy, professeur d'anatomie, de physiologie, agrégé à la Faculté de Médecine de Paris, chirurgien du Bureau central des hôpitaux, &c.

Réflexions sur la vaccine et la variole, ayant pour but d'obtenir, par la vaccination, l'extinction complète de la petite-verole; par J. A. Brisset, D.M.P. Paris, un vol. 8vo.

Synopsis of Nosology. By W. Cullen, M.D. to which is added, the classification of Diseases by Dr. John Mason Good. 18mo. pp. 151.

Medical Guide to Paris, &c. Translated from the French of M. Ratier, by J. R. Alceek. 18mo. pp. 141.

On the Origin and Progress of various changes of Structure in Man and some inferior animals. By Dr. J. Baron. 4to. with plates.

* A gramme is about 15 grains troy.

† A livre is equal to 15 ounces and 6 drachms troy, and a grain, French, is equal to 4-5 of a grain troy.